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THE MEASUREMENT OF ATTENTION[1]

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I. Introduction

In his *Postulates of a Structural Psychology* (1898) Titchener expressed the opinion that *clearness* is to be considered

[1] From the Psychological Laboratory of Cornell University.

as an intensive attribute of sensation, and that this attributive clearness is the elementary phenomenon of attention.[2] The same view is worked out in detail, and with reference to current theories of attention, in the *Feeling and Attention* of 1908.[3] In the following year, Geissler published "an attempt at a new measurement of attention in terms of clearness values."[4] "A very close parallelism was found to exist between introspectively distinguishable variations of attention and corresponding differences in the precision of work performed at these levels, under the condition that the estimation of degrees of attention was made in terms of clearness and that the work itself was not influenced by anything else but change in attention."[5] The attempt thus yielded a positive result. Geissler, however, worked only with material which was visually presented; and it is clear that the investigation must be carried further into the domains of hearing, touch, and imagery. The present study is an attempt to do for audition what Geissler has done for vision. Since Geissler prefaced his experiments by a fairly full "critical study of previous views and methods,"[6] we may ourselves dispense with any longer introduction. And since this paper deals only with audition, and experiments upon touch and imagery are still to follow, we have thought it best to offer our results by themselves, without criticism of studies which have appeared since Geissler wrote, and without reply to the occasional criticisms of Geissler's method which we have met with. The results which we have obtained confirm those of the previous investigation, so that we find no reason to change our fundamental view of the nature of the elementary phenomenon of attention; and the discussion of minor points of controversy may be postponed until the experimental material covers a wider ground.[7]

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[2] *Philosophical Review*, vii., 1898, 461 f. See also M. W. Calkins, *Introduction to Psychology*, 1901, 137 ff.; I. M. Bentley, *Mind*, N. S. xiii., 1904, 242 ff.

[3] *Lectures on the Elementary Psychology of Feeling and Attention*, 1908, Lects. i., v., vii.

[4] This JOURNAL, xx., 1909, 502 ff.

[5] *Ibid.*, 529. It may be noted that Geissler anticipates an obvious criticism by the statement: "under the same conditions, the introspective [we should prefer to say 'subjective'] estimation of the quality of the work was not as reliable as the evaluation of the degrees of attention."

[6] *Ibid.*, 473 ff.

[7] The writer takes this opportunity to thank Professor Geissler for important suggestions with regard to the method of the experiments reported below.

Observers.—Three observers served in all of the experiments: Dr. W. S. Foster (F), instructor in Psychology, Miss Mabel E. Goudge (G), graduate student in Psychology, and Mr. J. S. Johnston (J), fellow in Psychology. G and J worked without knowledge of our problem. F was familiar with Geissler's investigation, and knew in general the aim of the present study.

II. Methods and Results

A. *Preliminary Training in Introspection*

Our first problem was to familiarise our observers with differences of attributive clearness.

To this end a large number of preliminary experiments was given them. The experiments were of two kinds: with attention directed to the stimulus, and with attention directed away from the stimulus and upon some mental task. In the experiments of the first kind, two metronomes were set going at the rates of 100 and 120 strokes per minute respectively, and the observer, who sat with his back toward them, was instructed to direct his attention to the sounds, and to count the number of sounds between coincident strokes. The observation began with a signal, and ended after a minute and a half, with the word *Introspect.* The observer was then required to describe, in as great detail as possible, the pattern of consciousness during the period of observation. In the experiments of the second kind, the same stimuli were used, but the observer was required to perform some mental task, such as continuously adding, subtracting, multiplying, dividing, reciting, singing, or repeating the alphabet backwards. At the end of a minute, or a minute and a half, the observer was asked, as before, to give as full and complete an introspection as possible. These two kinds of experiments were alternated throughout the preliminary training; four observations were taken in an hour. The observer F gave in all 62 introspections; G, 60; and J, 128. After relatively few trials, the observers were able not only to compare the clearnesses of the stimuli in the two types of experiments, but also to say that the sounds were not always equally clear or obscure during an observation. Thus, F reports, after an experiment in which the task was to add 7 continuously: "The sounds of the metronomes, as a series of discontinuous clicks, were clear in consciousness only four or five times during the experiment, and they were especially bothersome at first. They were accompanied by strain sensations and unpleasantness. The rest of the experiment my attention was on the adding, which was composed of auditory images of the numbers, visual images of the numbers, sometimes on a dark grey scale which was directly ahead and about three feet in front of me. This was accompanied by kinaesthesis of eyes and strains in chest and arms. When these processes were clear in consciousness the sounds of the metronomes were very vague or obscure." Similar reports were made, during the first three weeks of training, by G and J.

When this degree of proficiency had been reached, the observers were asked to estimate, first the larger, and later the finer differences in the clearness of the sounds. They constructed, independently, a

rough scale of five or six steps, from very clear to obscure; but they were presently able to assign a percentage value to the clearnesses. At first this gradation was difficult; and the observers, particularly F, felt uncertain of its correctness. As the preliminary training advanced, however, they grew more confident; and toward the end they were able not only to give an analysis of consciousness during the period of an observation, but also to estimate, without difficulty, the clearness or obscurity of the mental processes experienced. At the end of three months, they had worked out (with some suggestion from the experimenter, which, however, bore only upon uniformity of grades) the following scale:

1. 100-90%.....maximally clear.
2. 90-80%.....very clear.
3. 80-70%.....clear.
4. 70-60%.....fairly clear.
5. 60-50%.....fair.
6. 50-40%.....fairly vague.
7. 40-30%.....vague.
8. 30-20%.....very vague.
9. 20-00%.....obscure.

We give a single illustration. F, after an experiment in which the task was to repeat continuously the alphabet backwards, reports: "Repeated alphabet backwards two and a half times. Practice has made this task much easier than at first, so that it is easier for the distraction (the metronomes) to catch attention. Four times for several seconds the sounds of the metronomes were the clearest processes in consciousness, perhaps 75% clear. The repeating process (*i.e.*, the complex of repeating the alphabet backwards, which was composed of visual images of dark grey lines stretching from right to left, eyes following along on lines sometimes with vague visual images of several letters in a group; accompanied by these visual images, or where visual images were lacking accompanied by the kinaesthetic images of eye movements, sibilant auditory images of letters) was only 10-20% clear. Aside from these four times the repeating processes were 85% clear, and the sound of the metronome was 10-15% clear."[8]

At this point we turned to the main problem of measuring attention in terms of clearness; in other words, of discovering how closely the attributive clearness of the processes attended to and of those attended from is correlated with quantitative and qualitative changes of an auditory stimulus of an objectively measurable character.

B. *Preliminary Practice in Observation Under the Conditions of the Main Experiments*

1. *Apparatus.*—Our stimulus, the tone of a Stern variator, varied from 300 to 600 vs. in the 1 sec. The Whipple air-tanks were used to supply the blast. The intensity of the tone was controlled by an air-valve, and the pitch, by the

[8] With this account *cf.* Geissler, *op. cit.*, 510 f.

crank of the variator. Both valve and crank were fitted with large scales and long moving arms, which permitted us to make gross movements in their adjustment.

The Whipple tanks[9] were devised for the purpose of giving a constant air-pressure; but further to insure this result we registered the pressure graphically. Between the air-valve and the variator a T-tube was inserted, the one arm of which led to the variator and the other to a large eosin manometer. A delicate Marey tambour was attached to the manometer, and its writing point rested upon the surface of a smoked drum. When the air-blast was turned on, the pressure was recorded upon the drum. The tanks as set up by Whipple showed a very slight variation, due perhaps to the small size of the intake valve. We therefore disconnected the tanks, and tried them individually. We found that the pressure was now sensibly constant; indeed, between the limits of height 25 and 50 cm., it remained, by our manometer, absolutely the same. During a single experiment the tank was therefore always kept within this central region. Curtains, hung directly over the variator and at other places about the room, eliminated echoes; and thus the constancy of the tone was further insured.

The observer sat about 2.5 m. from the source of sound, his head secured in position by a biting-board, and his right hand resting upon a silent electrical key. In an adjacent room was placed a kymograph with three writing points. One of these points was connected to the observer's key, another to an electrical push-button in the experimenter's hand. The third point, the lever of a Jacquet chronoscope, wrote fifths of seconds between the other two. It was added to give an approximate record of the observer's reaction-times.

2. *Instructions.*—The following instructions were read to the observer:

"You are to sit at the table with your back to the stimulus, your head held firmly in position by the mouth-piece and the biting board, your right arm and hand resting upon the table, and your forefinger, or forefinger and thumb, lightly pressing the electrical key. The experiment will begin upon the signal *Ready, Now,* and will end when the experimenter says *Introspect.*

"The stimulus, which will be the tone of a Stern variator, may vary in intensity or in pitch. The rate of change from one intensity to another, or from one pitch to another, will also vary, the change being made either gradually or very quickly. There are then two points upon which you will report: 1. kind of change, whether of pitch or intensity; and 2. rate of change, whether rapid or slow. As soon as a change is perceived, you will press the key.

"One pressure denotes a change of intensity;

"Two a change of pitch;

"The rate of change must be given in the introspection.

"You are to give your attention to the sound of the variator. At the end of 30, 45 or 60 seconds, as the case may be, you will introspect, and give a detailed description of your consciousness during the experiment."

[9] G. M. Whipple, this JOURNAL, xiv, 1903, 107 ff.

It will be observed that no mention is here made of the word "clearness." This omission was made advisedly; for we wished, so far as possible, to keep the observers in ignorance as to the subject of the experiment. It is true that *F* knew, in a general way, the aim of this study; but neither he nor the other observers knew the particular phase of the problem that we were studying. We expected, however, in view of the long preliminary training, to receive, without requesting them, detailed introspections upon the relative clearness of the processes in consciousness. In this expectation we were not disappointed; for both G and J continued to estimate the relative clearness of their mental processes in percentage terms, while F, throughout these experiments, used such descriptive terms as very clear, fairly clear, vague, obscure, etc.

In the experiments of 30 seconds, only one or two changes in pitch or intensity were made, while in the experiments of 45 and of 60 seconds, three and four changes in pitch or intensity were made respectively. A 'change' means a variation either of pitch or of intensity; not of both together. The rate at which the change was effected was either rapid or slow. The rapid changes were made as quickly as the adjustments allowed, requiring about one-fifth of a second; the slow changes occupied three seconds.

3. *Series.*—In the series given to the observers all the possible types of change were represented. In the experiments, *e.g.*, in which only one change was made during an observation, the change was in one case of pitch, in another of intensity; in one case it was made rapidly, in another slowly. The standard pitch and intensity were also varied, as was the place of the change within the 30 second interval (near beginning, middle, near end). Again, in the experiments in which four changes were made, the changes of pitch and of intensity occurred an equal number of times in the first, second, third, and fourth places. The rates of change, rapid and slow, likewise occurred an equal number of times in the first, second, third, and fourth places. The series itself varied from four changes of pitch to four changes of intensity.

The series in detail were as follows:

Series I. One change. Duration of experiment 30 seconds.

Variation.	Change of Pitch.	Change of Intensity.	Rate.
1.	0 (350)	1 (l-h)	r.
2.	0 (400)	1 (h-l)	s.
3.	0 (450)	1 (h-l)	r.
4.	0 (300)	1 (l-h)	s.
5.	1 (350-400)	0 (low)	r.
6.	1 (300-400)	0 (low)	s.
7.	1 (500-400)	0 (high)	s.

Series II. Two changes. Duration of experiment 30 seconds.

Variation.	Changes of Pitch.	Changes of Intensity.	Rates.
1.	0 (350)	2 (l-h-l)	s.r.
2.	0 (450)	2 (h-l-h)	s.r.
3.	0 (500)	2 (l-h-l)	r.s.
4.	1 (350-450)	1 (h-l)	s.r.
5.	1 (500-400)	1 (h-l)	r.s.
6.	1 (300-400)	1 (l-h)	r.r.
7.	2 (300-400-500)	0 (high)	r.s.
8.	2 (400-350-300)	0 (low)	s.s.

Series III. Three changes. Duration of experiment 45 seconds.

Variation.	Changes of Pitch.	Changes of Intensity.	Rates.
1.	0 (500)	3 (l-h-l-h)	r.r.s.
2.	0 (400)	3 (h-l-h-l)	s.r.s.
3.	1 (300-400)	2 (l-h-l)	r.r.s.
4.	1 (500-400)	2 (h-l-h)	s.s.r.
5.	1 (450-350)	2 (h-l-h)	s.s.r.
6.	2 (400-350-450)	1 (l-h)	r.s.r.
7.	2 (400-500-450)	1 (h-l)	r.r.s.
8.	2 (400-300-450)	1 (l-h)	s.r.s.
9.	3 (300-400-300-400)	0 (high)	r.s.s.
10.	3 (400-300-400-300)	0 (low)	s.r.r.

Series IV. Four changes. Duration of experiment 60 seconds.

Variation.	Changes of Pitch.	Changes of Intensity.	Rates.
1.	0 (350)	4 (l-h-l-h-l)	s.r.s.r.
2.	0 (400)	4 (h-l-h-l-h)	r.s.s.r.
3.	1 (350-450)	3 (l-h-l-h)	r.s.r.r.
4.	1 (450-350)	3 (h-l-h-l)	s.s.r.s.
5.	1 (500-400)	3 (l-h-l-h)	s.r.r.s.
6.	1 (400-450)	3 (h-l-h-l)	r.s.s.r.
7.	2 (500-400-350)	2 (l-h-l)	s.s.r.r.
8.	2 (350-450-350)	2 (l-h-l)	r.s.s.r.
9.	2 (350-450-500)	2 (h-l-h)	s.r.r.s.
10.	2 (350-400-450)	2 (h-l-h)	s.r.r.r.
11.	2 (500-400-300)	2 (l-h-l)	s.s.r.r.
12.	2 (400-500-400)	2 (h-l-h)	r.s.r.r.
13.	3 (400-300-400-500)	1 (l-h)	s.r.r.s.
14.	3 (300-400-500-400)	1 (l-h)	r.s.s.r.
15.	3 (400-300-400-300)	1 (h-l)	r.s.s.r.
16.	3 (300-400-450-500)	1 (h-l)	s.s.r.s.
17.	4 (450-500-450-400-300)	0 (low)	r.s.r.r.
18.	4 (400-350-450-500-550)	0 (high)	s.r.s.s.

The figures in parentheses under *Changes of Pitch* show the vibration-rates of the tones employed. The letters in parentheses under *Changes of Intensity* show the direction of change, from lower to higher, or conversely. The letters *r* and *s* under *Rates of Change* show the rates at which the changes were effected, *r* signifying *rapid* and *s, slow.*

There were thus 125 changes, 61 of pitch, and 64 of intensity. Of these 125 changes, 62 were made slowly and 63 rapidly.[10]

In the preliminary practice, variations of the above series were chosen at random. For a given experiment, *E* set the variator and the air-valve at the points required. On the signal *Ready, Now, E* released a spring-clip that closed the rubber tube between the air-valve and the variator, and at the same time pressed the push-button held in his hand. At every subsequent change, and at the end of the experiment, *E* again pressed the push-button; so that, by comparison with the time-line and with *O*'s line, the length of the experiment, the times of change, and the time of *O*'s reaction were graphically recorded.

F gave in all 52 introspections; G, 56; and J, 112. This preliminary practice covered the three months February to April, 1912. At its completion we turned to the main experiment, using the Single Task Method.

C. *Single Task Method*

1. *Apparatus.*—The same apparatus was used as for the preliminary practice; but the room, which before was light, was now darkened. The observer was, moreover, enclosed in a muslin booth, which was illuminated from above by an electric light, controlled from the experimenter's desk. The experiments were conducted in darkness, unless flicker was used as a distractor; and the light was turned on at the end of the experiment. *O* was thus able to write his introspections, while *E* marked the record and set the apparatus for the succeeding experiment.

Distractors.—In the preliminary practice the experiments were performed without distraction. In the present series, eight distractors were employed:

1. Flicker (9 rhythms and 4 intensities).
2. Electrical current (3 intensities).
3. Flicker and current (with above variations).
4. Clicks of single metronome (3 rates; 60, 100, and 150).
5. Clicks of metronome and current (with above variations).
6. Clicks, flicker, and current (with above variations).
7. Two metronomes beating at different rates (60, 100, and 150).
8. Phonograph.

(1) *Flicker.*—Behind and somewhat above the observer was placed a second electric light, enclosed within a reflector which directed the light upon a white screen in front of the observer. This light was also controlled from the experimenter's desk; it had four variations in intensity, from very weak to maximally strong. A large cardboard disc., from which were cut four sectors of 30°, rotated before

[10] The changes are indicated under the separate headings: thus, Series I yields 3 changes of pitch and 4 of intensity; and so forth.

the reflector. Cardboard shields adjustable over the sectors made it possible to obtain nine different rhythms in the flicker. Since there were four variations of intensity, and nine in rhythm of the light, there were altogether 36 variations of the flicker.

The cardboard disc that served as shutter was driven by a motor, which because of its noise was placed in an adjoining room. The motive power was transmitted by a reduction-system of string belts and gutta-percha pulleys to a large wheel, upon the shaft of which the shutter turned. As the light was controlled from the experimenter's desk, this part of the flicker apparatus, since it was noiseless, was started at the beginning of the hour and ran throughout the entire period.

(2) *Current.*—The faradizing current was also controlled from the experimenter's desk. The induction-coil was placed in an adjacent room. The strength of the primary current was governed by a three-way switch connected with a rheostat. This gave three intensities of the induced current. The current ran direct from the coil to the electrodes, which were moistened and applied to the observer's left arm, the one a little below the elbow, and the other at the wrist. During the first few experiments, the electrodes were bound one on each wrist; this plan was, however, abandoned because the strongest current caused a violent contraction of the muscles of the arm and hand, which seriously interfered with the observer's reaction. The contraction was none the less severe after the change to the left arm; but the right hand could now operate the key without hindrance.

(3) *Flicker and current.*—The third distractor was formed by the combination of the first two, and varied within their limits. There were therefore 108 possible variations of this distractor.

(4) *Metronome.*—Two metronomes were placed upon the experimenter's desk, and were there controlled. In the case of the fourth distractor only one metronome was used. The rate of the beat varied from 60 to 100 and 150 strokes in the minute.

(5) *Metronome and current.*—The fifth distractor was formed by the combination of the clicks of a single metronome and the electrical current. It was variable within the limits of these two distractors.

(6) The sixth distractor, formed by the combination of flicker, clicks of a single metronome, and electrical current, varied within the limits of these distractors. In all, 324 variations were possible.

(7) *Two metronomes.*—Both metronomes were set going at different rates, 60, 100 or 150. There were therefore only three variations within this form of distraction.

(8) *Phonograph.*—The phonograph was likwise controlled from the experimenter's desk. Various records were used, including popular and classical pieces, songs, instrumental and band music.

Previous experimenters have found that distractors very soon lose their power of compelling the attention; the observers become habituated. It was for this reason that we selected eight distractors which were capable of variation and extension. During the first few experiments those forms

of distraction were employed which we supposed to be the least disturbing. Habituation was in some measure counteracted by increase of intensity, or by change in rhythm of the distractor. Thus in the case of the flicker, and also of the electric current, the weaker intensities were used first, the stronger later.

2. *Instruction.*—The general instruction remained unchanged. The specific directions were as follows:

"The stimulus, which will be the tone of a Stern variator, may vary in intensity or in pitch. The rate of change from one intensity to another, and from one pitch to another, will also vary, the change being made either gradually or very quickly. There are then two points upon which you will report: 1. kind of change, whether pitch or intensity; and 2. rate of change, whether rapid or slow. As soon as a change is perceived you will press the key.

"One pressure denotes a change of intensity;

"Two a change of pitch;

"The rate of change must be given in the introspection.

"You are to give your attention to the sound of the variator, and to neglect as far as possible any distraction, purposive or accidental. At the end of 30, 45, or 60 seconds, as the case may be, you will introspect. In the general description of consciousness, in previous experiments, you have among other things assigned clearness values to the various processes reported. You are now to estimate clearness values only: that is, you are to report, using a scale of 100, the relative clearness of the processes which you observe in consciousness. For example: 'First change, rapid, clearness of so-and-so *x*%, of so-and-so *y*%; second change, slow, clearness of so-and-so *m*%, of so-and-so *n*%.'"

3. *Number of experiments.*—The experiments by this method were conducted in May and June 1912, and in February and March 1913. Each observer gave in all 86 introspections.

4. *Series.*—The series employed were those outlined above. The order of presentation was determined by lot. Each series was employed twice. Five or six observations, one of which was taken as a control without distraction, were made during the hour. The experiment without distraction occurred an equal number of times in the first, second, ... and sixth places. The distractions were also so distributed that each kind occurred but once in an hour, and as often in the first as in the other positions. During a single experiment the same distractor persisted without change.

5. *Results.*—The Single Task Method was employed, as we have said under 3 above, at two periods. During the interval the Double Task Method was employed. The results of G and F for the two periods of the Single Task Method

agree throughout, and are therefore grouped together. J, however, gave different results in the two periods; and they are therefore considered separately. J_1 denotes the results of the first, and J_2 those of the second period. The observer had for some time been depressed by a visual disability which oculists had failed to overcome, and in the first period was anxiously awaiting the outcome of a new treatment; this is probably the ground of the incapacity for high degrees of attention shown under J_1 in Tables I and II. In the second period, the reason for depression had been removed. It is possible, also, that the practice in concentration gained during the rather exacting observations of the Double Task Method helped the observer to give the improved results under J_2. We cannot offer more than this general explanation of the discrepancy.

In some of the experiments, from the nature of the series, one change was made, in others two, three, and four. In working over the results, each change was considered separately, was judged as a single case and so recorded. Hence in Table I only the *number of such cases* is given. Under *Right* are grouped all the right judgments of kind and rate of change, and under *Wrong* all the wrong judgments. The scale of attention in terms of clearness is arranged above.

TABLE I

NUMBER OF CASES, KIND AND RATE OF CHANGE

Report	Change	O.	RIGHT						WRONG					
			10-9	9-8	8-7	7-6	6-5	5-4	10-9	9-8	8-7	7-6	6-5	5-4
KIND AND RATE	Ps	F	33	14	2									
		G	2	22	9	5	2			2				
		J_1	2	5	5	3		1						
		J_2	9	9	1	1			1		2			
	Pr	F	32	5										
		G	3	18	11	8	1				2			1
		J_1		2	4	1	2							
		J_2	18	5	4	2	1							
	Is	F	13	11										
		G	2	19	6	2		1						
		J_1		3	6	1	2							
		J_2	4	5	3	2	1							
	Ir	F	27	10										
		G	2	13	15	3	3							
		J_1		2	7		2	1		1	1		1	
		J_2	6	3	1						1		1	

TABLE I—*Continued*

Report	Change	O.	Right						Wrong					
			10-9	9-8	8-7	7-6	6-5	5-4	10-9	9-8	8-7	7-6	6-5	5-4
KIND	Ps	F	1	2										
		G		1	1	4	1			1	3	4	1	
		J_1		1		2	2					2		
		J_2	2		2	1			1	1				
	Pr	F	4	9	1				1					
		G		2	1	4				1	2	3	1	
		J_1		1	1	6	2	1		1				
		J_2	1	3	2	1								
	Is	F	2	4	1					4				
		G		2	6	3	1					1		
		J_1		1		2	1	2					1	
		J_2	2	1										
	Ir	F	3	6	3				1	3		1		
		G		3	7	3	2					1		
		J_1		1	7		3							
		J_2	3	4	3		1	1	1					
RATE	Ps	F							1	2				
		G		1	3	4	1			1	1	4	1	
		J_1				2				1		2	2	
		J_2	1	1					2		2	1		
	Pr	F	1						4	9	1			
		G		1	2	3	1			2	1	4		
		J_1		1						1	1	6	2	1
		J_2							1	3	2	1		
	Is	F		4					2	4	1			
		G				1				2	6	3	1	
		J_1					1			1		2	1	2
		J_2							2	1				
	Ir	F	1	3		1			3	6	3			
		G				1				3	7	3	2	
		J_1								1	7		3	
		J_2	1						3	4	3		1	1

TABLE I—*Continued*

Report	Change	O.	Wrong 10-9	9-8	8-7	7-6	6-5	5-4
SUBJECTIVE REACTION	Ps	F	1	2				
		G				1		
		J_1						1
		J_2						
	Pr	F						
		G		2				
		J_1						
		J_2						
	Is	F	1	2	1			
		G				2	1	1
		J_1			1		1	
		J_2						
	Ir	F	1	8	1			
		G			3			1
		J_1						
		J_2						
NO REACTION	Ps	F						
		G						
		J_1						
		J_2				1		
	Pr	F						
		G						2
		J_1						
		J_2						
	Is	F	1	4	3	2	1	1
		G				8	6	4*
		J_1				1	2	1
		J_2		1	1	11		1
	Ir	F		1	1	1	1	
		G				3	4	1
		J_1					1	
		J_2				6	3	

* These four cases occurred in the region 4-3 (40-30% of clearness).

The observers' reports may, however, be grouped under five heads: 1. reports in which kind and rate of change are both right or wrong; 2. reports in which kind is judged aright, and rate is wrong; 3. reports in which rate is judged aright and kind is wrong; 4. reports of change in the absence of objective change (subjective reactions); and 5. reports which

failed to note an objective change. Under each one of these captions the data are further analysed, according as the objective change is of pitch or intensity, and is made rapidly or slowly. *Ps*, then, means that the objective change was a slow change of pitch; *Pr*, a rapid change of pitch; *Is*, a slow change of intensity; and *Ir*, a rapid change of intensity. In the case of the subjective reaction, the analysis depends upon the rate and kind of change reported by the observer.

In Table II the right cases, the wrong cases and the failures are grouped according to the kind and rate of the objective change. The subjective reactions are grouped as they were reported by the observer. A summary of the entire number of cases appears at the end of the table. It is clear that the greatest number of right cases occurs with the higher degrees of attention, and that the greatest number of wrong cases occurs from one to two steps lower upon the scale of attention. That this ordering is due to attention, and that it is not the result of any specific reaction to one kind or one rate of change, can be seen by referring to the separate captions of the tables. The relation of the right cases to the wrong cases for all changes, whether of pitch or intensity, whether rapid or slow, is remarkably constant.

TABLE II

NUMBER OF CASES GROUPED ACCORDING AS THE CHANGE IS OF PITCH OR INTENSITY, AND IS RAPID OR SLOW, WITH SUMMARY

Kind of Rate	Report	O.	Clearness						
			100-90	90-80	80-70	70-60	60-50	50-40	40-30
PITCH	Right	F	70	30	3				
		G	5	43	22	21	4		
		J_1	2	9	10	12	6	2	
		J_2	30	17	9	5	1		
	Wrong	F	1						
		G		4	7	7	2	1	
		J_1		1		2			
		J_2	2	1	2				
	Subjective	F	1	4					
		G					1		
		J_1						1	
		J_2							
	No-reaction	F							
		G						2	
		J_1							
		J_2				1			

TABLE II—*Continued*

Kind or Rate	Report	*O.*	Clearness						
			100-90	90-80	80-70	70-60	60-50	50-40	40-30
INTENSITY	Right	F	45	31	4				
		G	4	37	34	11	6	1	
		J_1		7	20	3	8	3	
		J_2	15	13	7	2	2	1	
	Wrong	F	1	7		1			
		G				2			
		J_1		1	1		2		
		J_2	1		1		1		
	Subjective	F	2	10	2				
		G			3	2	1	2	
		J_1			1		1		
		J_2							
	No-reaction	F	1	5	4	3	2	1	
		G				11	10	1	4
		J_1				1	3	1	
		J_2		1	1	17	3	1	
SLOW	Right	F	61	18		1			
		G	4	42	18	12	3	1	
		J_1	2	9	11	6	2	1	
		J_2	14	15	4	3	1		
	Wrong	F	7	15	4				
		G		5	7	7	2		
		J_1		2		4	3	2	
		J_2	5	1	4	1			
	Subjective	F	1	10	1				
		G				2	2	1	
		J_1			1		1	1	
		J_2							
	No-reaction	F		1	1	1	1	1	
		G				8	6		4
		J_1				1	2	1	
		J_2		1	1	12		1	

TABLE II—*Continued*

Kind or Rate	Report	O.	Clearness 100-90	90-80	80-70	70-60	60-50	50-40	40-30
RAPID	Right	F	46	29	2				
		G	5	32	28	15	5		
		J_1		4	11	1	5	1	
		J_2	25	8	5	2	1		
	Wrong	F	3	6	1				
		G		5	10	7	2	1	
		J_1		3	9	6	6	1	
		J_2	4	7	6	1	2	1	
	Subjective	F	2	4	1				
		G			3			1	
		J_1							
		J_2							
	No-reaction	F	1	4	3	2	1	1	
		G				3	4	3	
		J_1					1		
		J_2				6	3		
SUMMARY	Right	F	222	108	9	1			
		G	18	154	102	59	18	2	
		J_1	4	29	52	22	21	7	
		J_2	84	53	25	12	5	1	
	Wrong	F	12	28	5	1			
		G		14	24	23	6	2	
		J_1		7	10	12	11	3	
		J_2	12	9	13	2	3	1	
	Subjective	F	6	28	4				
		G			6	4	4	4	
		J_1			2		2	2	
		J_2							
	No-reaction	F	2	10	8	6	4	2	
		G				22	20	6	8
		J_1				2	6	2	
		J_2		2	2	36	6	2	

Inspection shows, further, that the subjective reactions occur for the most part under a relatively high degree of clearness, and conversely that the failures, the no-reactions, occur under a relatively low degree of clearness. We may infer that a subjective reaction is, under our conditions, a less serious error than a no-reaction. Emphasis was laid, throughout the experiment, upon reaction to change of the

tone of the variator, and the observer was 'set' for change; it is consequently but natural that he should sometimes 'imagine' a change; whereas failure to notice a change objectively presented argues a definite lapse of attention.[11] In view of these considerations, we have ventured to 'weight' our results as follows:

A right or wrong judgment of Kind and Rate counts as.... ±2.0
A right or wrong judgment of Kind counts as..... ±1.0
A right or wrong judgment of Rate counts as..... ±1.0
A Subjective reaction counts as.....—2.0
A No-reaction counts as.....—2.5

In Table III the results appear as thus weighted.

TABLE III

WEIGHTED SUMMARIES

O.		CLEARNESS						
		100–90	90–80	80–70	70–60	60–50	50–40	40–30
F	Total right	222.0	108.0	9.0	1.0			
	Total wrong	20.5	68.5	19.0	8.5	5.0	2.5	
G	Total right	18.0	154.0	102.0	59.0	18.0	2.0	
	Total wrong		14.0	30.0	54.5	35.0	13.5	10.0
J_1	Total right	4.0	29.0	52.0	22.0	21.0	7.0	
	Total wrong		7.0	12.0	14.5	20.5	7.5	
J_2	Total right	84.0	53.0	25.0	12.0	5.0	0.5	
	Total wrong	12.0	11.5	15.5	47.0	10.5	3.5	

The crest of the curve of right judgments now falls, with F, 1 place; with G, 2 places; and with J, 2 and 2 places respectively to the left, that is, above the crest of the curve of wrong judgments; and we have evidence, once more, that introspectively distinguished variations of clearness are closely paralleled by corresponding differences in the accuracy of the judgments passed.

The relation of the report to the kind and rate of the objective change appears in Table IV.

[11] Of the 1400-odd cases reported in these tables only 61 were subjective. We regard this small number (about 4%) as evidence both of the quality of our observers and of the accuracy and reliability of our apparatus and method.

TABLE IV

RELATION BETWEEN THE OBSERVER'S REPORT AND THE KIND AND RATE OF OBJECTIVE CHANGE, EXPRESSED IN NUMBER OF CASES

O.	KIND								RATE							
	Pitch				Intensity				Slow				Rapid			
	R.	W.	S.	N.	R.	W.	S.	N.	R.	W.	S.	N.	R.	W.	S.	N.
F	103	1	5		80	9	14	16	77	10	7	12	80	26	12	4
G	95	21	1	2	93	2	8	26	80	21	5	18	85	25	4	10
J_1	41	3	1		41	4	2	5	31	11	3	4	22	25		1
J_2	62	5		1	40	3		23	37	11		15	41	21		9
Total	301	30	7	3	254	18	24	70	225	53	15	49	228	97	16	24

R., right report. S., subjective report.
W., wrong report. N., no report.

Here we see, in spite of individual differences, first, that a change of pitch, under the conditions of our experiment, is more compelling than a change of intensity; and, secondly, that a rapid change of stimulus is more attractive than a slow change.

Our observers are more correct as regards changes of pitch than as regards changes of intensity. F, J_1, and J_2 report more errors for intensity than for pitch. G, on the other hand, reports more errors for pitch than for intensity. This difference, however, is more than offset by the greater number of cases in which she failed to observe a change of intensity. A like failure to apprehend changes of intensity is evinced by F, J_1, and J_2. F and J_1 noted "change" for every variation in pitch, and J_2 for every variation but one; while they failed to observe "change" in 16, 5, and 23 cases, respectively, of change in intensity.

Slow changes are, as Stern has noted, "less likely than rapid to cause a reaction of attention." This rule is borne out, in a measure, by our results. The objective change was neglected 44 times when it was made slowly, and only 24 times when it was made rapidly. Moreover, the change when rapidly effected was reported correctly 228 times, and when slowly effected, 225 times. On the other hand, more wrong reactions were made to rapid changes (97 reported as slow)

than to slow (53 reported as rapid). The law, however, rests upon a fairly large body of experimental results.[12]

(1) *The promptness of voluntary action, i. e.,* the time of a simple reaction, was first used to express degrees of attention by Obersteiner in 1879.[13] He employed two distractors, an induction current and a musical box, and found that the observer's reaction was slower under distraction. He therefore assumed that "this retardation stands in inverse proportion to the intensity of attention." "The differences in the reaction period," he continues, "which may serve directly as the measure of attention, vary in different individuals, and in the same individual under different conditions."[14] Many investigators[15] have used the reaction method to measure the attention. Their results show, for the most part, that reaction-time increases with distraction, though there are those who deny the correlation. Cattell,[16] for example, found that there is no corresponding lengthening of the reaction-time with reduction of attention; and Geissler writes that "the final outcome of the reaction experiments, as used for the measurement of attention, has been on the whole negative; it has been impossible to establish a positive correlation between high degrees of attention and short reactions, and between lower degrees and correspondingly lengthened reactions."[17] In his own experiments, however, Geissler found a remarkably high correlation between "the observers' estimates of attentive concentration and the calculated quickness and accuracy of their results,"[18] and remarks that "with all three observers there is a perfect correlation between their best attention and their shortest time, and between correspondingly lesser degrees and longer times."[19]

Our apparatus was arranged, as we have said, to measure roughly the reaction-time of our observers. The average and the mean varia-

[12] G. E. Müller, *Zur Theorie der sinnlichen Aufmerksamkeit*, 1873, 125 ff.
A. Pilzecker, *Die Lehre von der sinnlichen Aufmerksamkeit*, 1889, 20 f.
W. James, *Principles of Psychology*, I., 1890, 416 f.
L. W. Stern, *Psychologie der Veränderungsauffassung*, 1898, 211 ff.
W. B. Pillsbury, *Attention*, 1908, 30.

[13] H. Obersteiner, *Brain*, I, 1879, 439-453.

[14] *Op. cit.*, 444.

[15] G. Buccola, *Rivista di filos. scientif.*, I, 219 ff.
G. S. Hall, *Mind*, VIII, 1883, 170-182.
W. Wundt, *Grundz. d. physiol. Psychol.*, I, 1874, 749 f.; II, 1887, 293 f.; III, 1903, 441 ff.
E. J. Swift, this JOURNAL, V, 1892, 1-19.
W. James, *Principles of Psychology*, I, 1890, 425, 427-434.
S. E. Sharp, this JOURNAL, X, 1899, 356.
A. Kästner and W. Wirth, *Psychol. Stud.*, III, 1907, 361-392; IV, 1908, 139-200.

[16] J. McK. Cattell, *Mind*, XI, 1886, 242.

[17] *Op. cit.*, 498.

[18] *Ibid.*, 508.

[19] *Ibid.*, 514.

tion of the reaction-times in the total number of experiments,[20] irrespective of the rightness or wrongness of the judgments, appear for each observer in Table V.

TABLE V

AVERAGE REACTION-TIME IN SECONDS AT THE DIFFERENT LEVELS OF ATTENTION

Observer	CLEARNESS											
	100–90		90–80		80–70		70–60		60–50		50–40	
	Av.	m.v.	Av.	m.v.	Av.	m.v.	Av.	m.v.	Av.	m.v.	Av.	m.v.
F	.9	.48	1.1	.55	2.0	1.1	1.0	.0				
G	.5	.3	1.9	.93	2.0	1.03	1.9	1.05	2.1	1.07	4.0	.0
J_1	.3	.1	1.1	.4	1.3	.9	1.3	.45	1.8	.66	1.6	.8
J_2	1.5	.7	1.8	.8	1.8	.83	2.3	1.2	2.3	1.9	2.6	.0

Av., average reaction time. m.v., mean variation.
Where the mean variation is given as 0, only one experiment occurred under that rubric.

There is evidently a positive correlation between attention, introspectively estimated in t rms of the attributive clearness of mental processes, and rate of reaction. In the whole table there are but four cases in which a lower degree of clearness gives a shorter average time than the next higher degree. Three of these may be summarily dismissed because they are the averages of too few cases: 1, 5, and 1 respectively. The fourth case, that under the fourth rubric of G, cannot be so disposed of. Neither can we consider it as an effect of practice, for this was minimized by the long preliminary series, while the several component reactions were also well distributed throughout the experiment. It is, perhaps, worthy of note, that the mean variation is high. In any case the exception is not of sufficient weight to affect seriously the coefficient of correlation as figured by Pearson's familiar "product moments" method.[21] Calculation yields the following results:

O	Correlation[22]	P.E.
F	0.95	0.038
G	0.76	0.118
J_1	0.94	0.032
J_2	0.98	0.048

This correlation is surprisingly high, when it is remembered that no emphasis was laid, in our instructions, upon the observers' reaction.

[20] The subjective reactions and the failures to react, *i. e.*, the "no-reactions," of course are not considered here; neither case gives a reaction-time.

[21] G. M. Whipple, *Manual of Mental and Physical Tests*, 1910, 27 f.

[22] In computing this correlation, the data under the fourth rubric for F, and the sixth rubric for G, J_1, and J_2, were omitted, for the reason that they represent too few cases: 1, 1, 5, and 1, respectively.

Moreover, no one of our observers, so far as we know, was aware that the reactions were being measured.[32]

(2) *The mean variation* has frequently been proposed as a measure of attention. A small mean variation would correspond to a high degree, and a large variation to a low degree of attention. Obersteiner was probably the first to suggest this correlation. He showed not only that the reaction-times were longer, but also incidentally that the mean variation was greater, in experiments made under distraction.[34] Later, Friedrich remarks: "It is tempting to fix definitely the somewhat unsettled concept of attention by making it proportional to the measure of precision, *i. e.*, to the reciprocal value of the average error, so that a small average error should correspond to a high degree of attention and, conversely, a large average error to a low degree of attention."[35] Although his results show close agreement with theory, he nevertheless is careful in his interpretation of them. He thinks that only "in the case of the simplest mental processes, which are as homogeneous as possible and but little subject to practice, may one assume that the average error is essentially dependent upon the degree of attention." Other authors, however, have been less cautious, and have insisted that attention may be measured by mean variation.[36]

Our own results tend to confirm this view. The increase of the m. v. in Table V is not due in any measure to fatigue or practice. It is not due to fatigue, for only five experiments of 30 to 60 seconds were conducted during an hour; and it is not due to practice, for this was raised to a maximum by the long practice series. In the entire table, with the exception of the levels at which only one experiment is reported, there are but two cases where a lower degree of attention has a smaller m. v. than the next higher. The mean variations for F and G increase uniformly with decrease of the attentive level. Those for J_1 and J_2 are less regular. Nevertheless, the lower degrees of attention still show a greater m. v. We seem justified then, even on Friedrich's principles, in drawing the conclusion that degree of attention can be introspectively estimated in terms of clearness.

(3) The *relation of the reaction-times to the kind and rate of the objective change* appears in Table VI.

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[32] In our case, therefore, the reactions were not known to be reactions, *i. e.*, were not made under the *Aufgabe* of reaction. It is possible that a "reaction" of this sort indexes attention, whereas a formal and set reaction, so understood by the reactor, is too complex a matter to serve as an index.

[34] *Op. cit.*, 446, 447.

[35] M. Friedrich, *Philos. Stud.*, I, 1883, 73.

[36] H. Griffing, this JOURNAL, VII, 1895, 235.
A. Oehrn, *Psychol. Arbeiten*, I, 1895, 92 ff.
V. Henri, *L'année psychol.*, III, 1896, 245.
J. J. van Biervliet, *Journ. de Psychol.*, I, 1904, 230.
A. Binet, *L'année psychol.*, XI, 1905, 71.
W. Peters, *Arch. f. d. ges. Psychol.*, VIII, 1906, 403 ff.

TABLE VI

RELATION BETWEEN THE REACTION-TIME IN SECONDS AND THE KIND AND RATE OF THE OBJECTIVE CHANGE

Observer	Kind				Rate			
	Pitch		Intensity		Slow		Rapid	
	Av.	m.v.	Av.	m.v.	Av.	m.v.	Av.	m.v.
F	.80	.28	1.27	.76	1.30	.60	.76	.42
G	1.70	.79	2.40	2.14	2.50	1.13	1.50	.75
J_1	1.18	.37	1.50	.58	1.60	.63	1.00	.35
J_2	1.41	.72	2.45	1.21	2.56	1.08	1.18	.56

Av., average reaction-time. m.v., mean variation.

The average reaction-time to a change of pitch is, under the conditions of our experiment, much less than that to a change of intensity; and, further, the average reaction-time to a rapid change is shorter than that to a slow change. This relation is constant for all observers. There is, to be sure, great individual variation, but this tallies in general with the character and quality of the observer's report. F, *e. g.*, gives an exceedingly low reaction-time, but, on the other hand, he makes few errors, and his attention is most frequently judged to be of the highest degree.

(4) A comparison of Tables IV and VI shows that there is a close *relation between reaction-time and accuracy of report; i. e.*, the shorter the reaction-time, the greater is the probability that the report is correct. Of the kinds of change, pitch has the shorter reaction-time, and also the greater number of right cases and the smaller number of failures. Of the rates of change, the rapid has the shorter reaction-time and also the greater number of right cases and the smaller number of failures. It would seem then that there is a direct relation between the quality and character of the report and the reaction-time.

This relation becomes apparent at once if the reaction-time is compared directly with the reports. Table VII shows the average

TABLE VII

THE REACTION-TIME IN SECONDS AND THE MEAN VARIATION OF THE RIGHT AND THE WRONG REPORTS

Observer	Right		Wrong	
	Av.	m.v.	Av.	m.v.
F	.93	.50	1.2	.63
G	1.96	1.06	2.3	1.23
J_1	1.28	.62	1.45	.60
J_2	1.71	.81	1.80	1.05

Av., average reaction time. m.v., mean variation.

and mean variation of the reaction-times for both the right and the wrong cases. It confirms the conclusions drawn from the comparison of Tables IV and VI.

The average reaction of the right reports for all of our observers is shorter than that of the wrong reports. While the difference is very small for J_2, it is so marked in the cases of J_1, and especially of G and F, that we may conclude with Whipple[77] that there is a close correlation between rate of judgment and character and quality of report.

(5) The *effect of the distractors* is shown in Table VIII. The average clearness of the focal processes (the sounds of the variator), its mean variation, and the number of cases are given for each distractor.

TABLE VIII

THE AVERAGE CLEARNESS OF THE AUDITORY SENSATION AS AFFECTED BY THE DISTRACTORS

Observer		Distractor								
		0	1	2	3	4	5	6	7	8
F	Av.	89.5	85.7	88.6	86.4	85.8	87.5	88.8	87.0	77.5
	m.v.	3.6	4.4	3.9	3.6	7.3	5.3	4.1	3.8	12.1
	No.	38	22	21	15	25	24	30	27	26
G	Av.	85.0	73.2	80.3	70.6	76.0	65.8	71.0	74.5	57.8
	m.v.	4.9	8.3	3.3	7.6	7.4	7.3	6.5	8.9	8.8
	No.	29	23	31	27	25	31	23	25	34
J_1	Av.	73.7	72.0	58.7	62.1	70.0	63.3	58.0	67.6	54.0
	m.v.	5.1	4.8	13.1	9.1	5.1	9.0	8.2	9.0	8.7
	No.	14	10	8	13	8	7	14	14	9
J_2	Av.	94.0	84.0	77.7	80.0	74.0	79.6	75.7	73.0	60.0
	m.v.	1.5	8.0	11.8	9.0	9.0	10.5	8.5	10.5	14.6
	No.	11	15	9	20	18	15	17	13	15

Av., average clearness of tone. m.v., mean variation. No., number of cases. 0, normal conditions. 1, flicker. 2, current. 3, flicker and current. 4, single metronome. 5, metronome and current. 6, metronome, flicker and current. 7, two metronomes. 8, phonograph.

[77] G. M. Whipple, this JOURNAL, XII, 1900-01, 433 ff. Whipple gave his observers two successive tones, the second of which varied between the relations equal, greater, or less by eight vibrations per second from the first. The second tone was judged in terms of the first. He divided the observers' judgments into three classes according as the report was immediate, slow, or deliberate. He defined the "immediate" as the judgment made without conscious comparison; the "slow," as the judgment made with conscious comparison; and the "deliberate," as the judgment in which the decision was the result of internal debate. He found that a much greater percentage of the immediate judgments was correct than of the slow or the deliberate.

It will be seen that the number of cases under the several distractors varies. This fact appears to confute the statement (made in the discussion of the method) that the distractors were used an equal number of times. But the discrepancy is explained by the nature of the series. In some, only one change was made, in others 2, 3, and 4. The distractors were to be employed an equal number of times, as many times first as last, etc., so that their order had to be prearranged. The order of the series, however, was not predetermined. Though every series was used an equal number of times, choice was made at haphazard. The series with the large number of changes thus accidentally fell to some distractors more frequently than to others. Still, on the whole, the variation in number of cases is slight and of little consequence.

There is wide variation in the effect of the distractors both upon the individual observers and upon the same observer at different times. F was little affected by any distractor save the phonograph. This result agrees closely, as we have said, with the general character and quality of his work. His attention during the experiments was higher, he made fewer errors, the reaction-time of his judgments was quicker, the mean variation smaller, than those of either of the other observers. The phonograph served to distract his attention; but even with it attention might be as high as under normal conditions. He could disregard it provided that unfamiliar pieces were played. When familiar records were played, his attention was "compelled" by them, he was "unable to attend from" them.

The phonograph proved to be the most efficient distractor for all the observers; and attention under normal conditions without distraction proved to be highest for all observers. This is shown in Table VIII; and also, perhaps more clearly, in Table IX, in which the effectiveness of the distractors is arranged in ascending order from least effective (normal conditions) to most effective (phonograph). In other respects, however, there is little or no agreement. G, J_1, and J_2 show, unlike F, a wide distribution of effectiveness; while F's average attention for all distractors, with the exception of the phonograph, lies within four degrees of clearness and is very nearly as high as under normal conditions.

TABLE IX

ORDER OF THE EFFECTIVENESS OF THE DISTRACTORS FROM LEAST TO GREATEST

Observer	Order								
	1	2	3	4	5	6	7	8	9
F	0	6	2	5	7	3	4	1	8
G	0	2	4	7	1	6	3	5	8
J_1	0	1	4	7	5	3	2	6	8
J_2	0	1	3	5	2	6	4	7	8

0, normal. 1, flicker. 2, current. 3, flicker and current. 4, single metronome. 5, metronome and current. 6, metronome, flicker and current. 7, two metronomes. 8, phonograph.

That the effect of the distractors is different for the same observer at different times is shown by J. Though his results for the two periods of the experiment show a gradual change in the effectiveness of the distractors, maximal attention under normal conditions, and minimal attention under the distraction of the phonograph, they can in no other detail be compared. In the first period the degrees of clearness of the focal processes ranged from 73.7 to 54.0; in the second period they ranged from 94.0 to 60.0. An explanation has been suggested above.

Table IX shows, further, that there is no uniform gradation of the distractors. Thus the flicker (no. 1), which next to the phonograph proved to be most effective for F, was only moderately effective for G, and had the least effect of all upon J. And so on with the rest.

It has usually been supposed that attention is best under slight distraction. Titchener says: "It has been shown experimentally that we attend best under a slight distraction,"[18] and Geissler finds that "the results of the second group of experiments showed plainly that even the most complex combinations of distractors, after a few days' work, were unable to induce great variations of attention. Instead, toward the end of the whole group, most of the normal series were actually performed at a slower rate than the distraction series."[19] Hamlin also gives a similar report. She used adding as a distractor, and remarks that "the subjects usually found that it acted as a spur rather than as a check to the attention."[20] Our results show, on the contrary, that attention is best under normal conditions; the distractors lower the attention. The advantage of the normal series is, in the case of F, not very great; but it is uniform, and in the cases of the other observers well-marked. Under the conditions of our experiment, therefore, the observers attended best under normal conditions.

Owing to the precautions that we had taken, the effect of *practice* and *habituation* in these experiments was practically negligible. In the first place, the work was divided into two parts, separated by a period of nine months; in the second place, a large number of distractors were employed, and these were capable of wide variation in intensity and in rhythm; and thirdly, as the experiments advanced, the intensity and rhythm of the distractors were proportionally increased and complicated. Habituation was therefore reduced to a minimum.

In choosing the distractors, our ideal was that of Drew, "to arrange a series of tasks of increasing degrees of complexity which should from the normal make ever greater demands on the mind until the attention should pass from a fully concentrated to a completely distracted state."[21] This is the principle laid down by Stumpf in his *Tonpsychologie*,[22] and by Titchener in his *Psychology of Feeling and Attention*.[23] The results show that we were not successful in obtaining such a series of graded distractors. There appear to be four main reasons. (1) A change in the stimulus may cause the corresponding conscious processes to rise *involuntarily* in clearness. It "catches" the attention. The tone may be comparatively obscure

[18] *Psychol. of Feeling and Attention*, 1908, 203.
[19] *Op. cit.*, 513.
[20] A. J. Hamlin, this JOURNAL, VIII, 1896, 49.
[21] F. Drew, this JOURNAL, VII, 1895, 533.
[22] C. Stumpf, *op. cit.*, I, 1883, 73-75.
[23] E. B. Titchener, *op. cit.*, 1908, 277, 278.

just before the moment of change, but at and during the change it may be maximally clear. (2) There are, in many of the distractors, brief *moments of interruption*, during which the change may occur and the reaction may be performed as if under normal conditions. (3) The distractors *vary in effect from individual to individual*. What may be a graded series for one observer would affect another very differently. (4) The distractors *do not even affect the same observer* in the same way from day to day. Factors are here involved which are subjective in nature (mood, general organic tonus, etc.), and which it is therefore extremely difficult to bring under objective control.

D. *Double Task Method*

1. *Apparatus.*—The Single Task Method involved only the higher degrees of attention. Under the instructions, this could hardly have been otherwise; with attention directed *to* the sounds of the variator and *from* the distractors, the tones would necessarily be of the higher degrees of clearness. It was incumbent upon us, therefore, to extend the work by some method which should induce the lower degrees of attention, and thus to discover if the conclusions so far drawn hold when the lower levels of attention are involved.

Geissler gives four essential requirements for such a method.[34] The efficiency of the work performed must depend as exclusively as possible upon the degree of attention given to it, and as little as possible upon such factors as practice, fatigue, mood, etc. The performance should never become entirely automatic or habitual. It must require absolutely continuous attention, so that a momentary lapse should at once manifest itself in a momentary reduction of the quality of the work. And the execution of the work must easily submit itself to a scale of quantitative gradation.

The method that seemed best to fulfill these requirements was the Double Task Method. This seems to have been first used by Loeb in 1886,[35] but since that time it has been employed successfully by many experimenters.[36] It requires the

[34] *Op. cit.*, 515.

[35] J. Loeb, *Arch. f. d. ges. Phys.*, XXXIX, 1886, 592-597.

[36] A. Binet *Rev. philos.*, XXIX, 1890, 138-155.
H. Münsterberg, *Beitr. z. exper. Psychol.*, IV, 1892, 200 ff.
W. G. Smith, *Mind*, N. S., IV, 1895, 50-73.
F. Drew, this JOURNAL, VII, 1895, 533-572.
V. Henri, *Année psychol.*, III, 1896, 232-278; VII, 1900, 250 ff.
J. C. Welch, *Am. J. Phys.*, I, 1898, 283-306.
W. McDougall, *Brit. J. Psychol.*, I. 1904, 435-445.
W. Wirth, *Psychol. Stud.*, II, 1906, 30 ff.
A. Kästner and W. Wirth, *Psychol. Stud.*, III, 1907, 361-312; IV, 1908, 139-200.
S. de Sanctis, *Zeits. f. Psychol.*, XVII, 1898, 205-214.
L. R. Geissler, *op. cit.*, 515-529.

observer to divide his attention, and to perform simultaneously two different mental tasks.

The first task which we selected was essentially the same as that of the previous experiment,—judging and reporting the changes of an auditory stimulus. The recording apparatus, the Stern variator, and the apparatus for the control of the tone, were the same as before. The second task consisted either of counting discrete objects, or of continuous adding.

In the counting of discrete objects, no. 12 BB shot were first used. Since the right hand rested upon the electrical key, the counting was done with the left. The shot proved to be too small to be picked up one by one and thus counted; and an apparatus was constructed whereby they rolled, as they were counted, down an inclined plane to a common receptacle. The rolling, however, made a slight noise; this gave rise to a distraction which under the conditions of the Double Task Method was objectionable; and accordingly a felt pad was substituted for the box. But now the observers had trouble in separating the shot, in moving them rapidly over the surface of the felt; so that, even under the best conditions, accurate records were seldom made. We therefore abandoned the shot in favor of small corks. These permitted of gross movements, could be taken up separately as counted, and were noiseless. They were placed upon a felt pad in front of the observer, and were transferred as counted to a felt-lined box which stood a few cm. to the left.

In the continuous adding, five series of thirty figures were selected to constitute five different degrees of difficulty. The easiest contained all the figures from 1 to 9; with the exception of figure 3, which occurred six times, each integer appeared three times in the series. In the second series all the figures from 3 to 13, with the exception of 10, occurred three times. In the third series all of the figures from 13 to 23, with the exception of 20, occurred three times. In the fourth series the following figures 23 to 27 were used once; 33 to 37, 43 to 47 were used twice; and 53 to 57 once. In the fifth and last series the figures 63 to 67 were used once; 73 to 77 and 83 to 87 twice; and 93 to 97 once. By using a different starting-number, the effect of practice and memory of previous results were entirely eliminated. The starting-number varied in regular order from day to day between the odd numbers from 3 to 25.[87]

The numbers were presented visually by an exposure apparatus which was constructed from a kymograph drum. This was slowly revolved by the motor which, in the Single Task Method, actuated the flicker. The drum was concealed behind a neutral-gray screen which stood in front of the observer at a distance of about 30 cm. A rectangular slit 1 x 3 cm. was cut in the center of the screen directly in the observer's line of regard. The apparatus was so arranged that the numbers appeared from above. The rate of presentation was variable as slow, moderate, and rapid. It was controlled from the experimenter's desk by a three-way switch, and was governed by increase or decrease of the strength of the electrical current. The rate of presentation was also controlled by the spacing of the

[87] *Cf.* Geissler, *op. cit.*, 504.

figures upon the drum, which was constant throughout an entire experiment. In Series I the figures were single-spaced; in Series II, doubled-spaced; and in the other series, III, IV, and V, triple-spaced. The exposure slit was illuminated by an electric light which was fixed just above and somewhat behind the observer. The illumination was constant and uniform, and the observer's head cast no shadow upon the visual field.

A slight shift of the drum upon its shaft made it possible to expose a number-series of any degree of difficulty in the rectangular window. The easier series were first shown, variation occurring only in the starting-number and in the rate of presentation. As the observers became practised in addition, the more difficult series were gradually introduced. We sought to keep the mental task of such difficulty that a high degree of attention was required, and that a lapse in attention should manifest itself directly in the quality and character of the work. Great care, however, had to be exercised not to increase the difficulty of the series and the rate of presentation beyond the limits of the individual observer. F was the only observer to add successfully series V, even when presented at the slowest rate.

2. *Instruction.*—As a control, every third experiment was conducted under the normal conditions of the Single Task Method; *i. e.*, the sole task was observation of, and response to, the changes of the auditory stimulus. Five experiments were usually made during an hour. They began at the signal *Ready, Now,* and the observer directed his attention as instructed, either to the tone of the variator, or to the counting of the corks, or to the adding of the figures. The instruction was as follows:

"In this experiment you are to record all changes of intensity and of pitch as in the previous experiment, by one and two pushes respectively upon the key, and are also in the subsequent introspective reports to give the rate of change.

"You are to direct your attention (to the tone of the variator,) (to the counting of the corks,) (to the adding of the figures). At the end of the experiment, which may run for thirty seconds, you shall (give the number of corks you have counted,) (give the sum of the figures you have added,) answer the following questionary:[28]

1. How much attention in terms of clearness was given:
   a. To the auditory stimulus?
   b. To the other required task,—if there was one?
   c. To any other sensory or ideational processes which may have entered consciousness during the course of the experiment?
2. What affective mood prevailed during the experiment?
3. Have you any comment to make?"

[28] *Cf.* Geissler, *op. cit.*, 519.

These instructions were read to the observer before each experiment.[89] The three kinds of experiment, in which the attention is directed to different tasks, will henceforth be represented by the numerals 0, 1, and 2. 0 refers to the normal or check series, in which attention is directed to the sound of the variator; 1 refers to the Double Task Method, in which it is directed also to the counting of the corks; and 2 refers to the Double Task Method, in which it is directed also to the adding of the figures. These three kinds of experiments are taken an equal number of times, and occur as many times in the first place as the second, third, fourth, and last places.

3. *Number of experiments.*—The experiments were made during the autumn of 1912. The same observers who had taken part in the other series were, fortunately, available. F gave in all 152 introspections: G, 150; and J 151.

4. *Series.*—The series of the Double Task Method differed from those of the Single Task Method in two respects. First, the time of the experiments was reduced uniformly to 30 seconds. This reduction was thought advisable, since the shorter period tended toward accuracy in the introspective reports, and since it tended also to rest the efficiency of the work performed upon degree of attention, and as little as possible upon fatigue. Secondly, the number of changes within a single experiment was reduced to two. The four, and even the three changes which were introduced in the Single Task Method had there proved to be a source of difficulty; and we could not anticipate their successful use under the new conditions, in which the attention was divided between two tasks. In four of the series but one change was made. This precaution was taken in order that the observer might not become habituated to two changes. In all other respects, the series agreed with those of the Single Task experiment.

[89] The importance of repeating the directions is not to be overlooked. On one occasion, the observer was merely directed to "count corks" and not as usual to "direct attention to the counting." The effect on the experiment was plainly shown in the report and introspection. The report was correct, and the clearness was maximal. The introspection read: "At first attention divided equally between tone and counting, both on upper level. On the whole the tone was slightly clearer. Eyes right; turned toward source of sound. These two processes, tone and counting, on upper level for short time. Later they rapidly fluctuated; now the tone was clearer, now the counting."

In detail, the series were as follows:

Series	Time of change	No. of Pitch changes	Pitch of tone	No. of intensity changes	Intensity of tone	Rate of change
I	10″	1	400–450	0	h	r.
II	15″	1	350–450	0	l	s.
III	20″	0	450	1	h-l	r.
IV	25″	0	300	1	l-h	s.
V	5″–10″	2	500–400–500	0	h	r.r.
VI	5″–15″	2	400–500–400	0	l	s.s.
VII	5″–20″	2	500–400–300	0	h	r.s.
VIII	5″–25″	2	300–400–500	0	l	s.r.
IX	10″–15″	1	400–450	1	h-l	r.r.
X	10″–20″	1	500–450	1	l-h	s.s.
XI	10″–25″	1	300–400	1	l-h	r.s.
XII	10″–28″	1	500–400	1	h-l	s.r.
XIII	2″–28″	0	300	2	l-h-l	r.r.
XIV	15″–20″	0	500	2	h-l-h	s.s.
XV	15″–25″	0	400	2	l-h-l	r.s.
XVI	15″–28″	0	450	2	h-l-h	s.r.

SUMMARY

Kind of change	No. of changes	No. slow changes	No. rapid changes
Pitch	14	7	7
Intensity	14	7	7
Total	28	14	14

5. *Results.*—In working over the data, the observers' reports were grouped as in the other experiment. It soon became apparent, however, that this classification was not entirely adequate. There were occasions when the observer reported "change" without giving either kind or rate. Such cases did not occur with the Single Task Method. We dispose of them by grouping them under a new heading, as *Fact of Change.*

The results appear in Table X. The data from the check or normal experiments are not here considered. Only one task was set in those experiments, and the attention was directed to the tone of the variator; consequently, the clearness of the auditory processes was nearly always maximal, and the reports were nearly always correct; the results add nothing to those of Tables I and II (Single Task Method), and are therefore omitted here. Our object in giving the normal series was, it will be remembered, not to confirm the results of the Single Task experiment, but merely to afford a means whereby the observer could, from time to time, compare the clearness of the auditory processes in concentrated and divided attention.

TABLE X

NUMBER OF CASES, KIND AND RATE OF CHANGE

Report	Change	O.	RIGHT									WRONG								
			10-9	9-8	8-7	7-6	6-5	5-4	4-3	3-2	2-0	10-9	9-8	8-7	7-6	6-5	5-4	4-3	3-2	2-0
KIND AND RATE	Ps	F	4	13	6	2								1					2	
		G		1	1	1			1							7	7	2	1	
		J						5	6	5								1	3	2
	Pr	F	4	10	4									1						
		G		1	1	17	7	2										1		
		J						10	15	6	1								1	
	Is	F	1	3	7							1	1		1					
		G		1			1	1								4	5	1	2	
		J						5	4	2	1									
	Ir	F	5	11	4	4							1			1				
		G			3	1		1	1							2	2	3		
		J						5	4	2	1									
KIND	Ps	F		3	5	1	3						1	1						
		G				4	8	3							1		1			
		J						1	6	7	1							4	4	
	Pr	F		5	8	7	1							1						
		G				1	1	1							1	1	2			
		J						3	9	5										
	Is	F		11	4	1	1							1						
		G			3	2	1	1		2										
		J						1	3	7										
	Ir	F			6	1							1	2						
		G			1					1					3	5	2	1		
		J						1	3	14									1	
RATE	Ps	F		1	1								3	5	1	3				
		G				1		1							4	8				
		J							4	4						1	3	6	7	1
	Pr	F			1								5	8	7	1				
		G				1	1	2							1	1	1			
		J															3	9	5	
	Is	F			1								11	4	1	1				
		G												3	2	1	1		2	
		J															1	3	7	
	Ir	F		1	2									6	1					
		G				3	5	2	1					1					1	
		J								1							1	3	14	
FACT OF CHANGE	Ps	F			1															
		G				1	1	1												
		J							1											
	Pr	F																		
		G			1															
		J						1												
	Is	F	1																	
		G						1	2											
		J							1		1									
	Ir	F																		
		G		1			1	1												
		J																		

TABLE X—*Continued*

Report	Change	O.	Wrong									
			10-9	9-8	8-7	7-6	6-5	5-4	4-3	3-2	2-0
SUBJECTIVE REACTION	Ps	F									
		G									
		J									1
	Pr	F									
		G									
		J									
	Is	F		1		1					
		G							1	2	
		J									1
	Ir	F	1		1	1					
		G								1	
		J									
NO REACTION	Ps	F									5
		G						1	2	1	7
		J									4
	Pr	F									2
		G							1	2	5
		J								2	2
	Is	F				1			1	4	16
		G						2	4	7	13
		J								2	16
	Ir	F								1	12
		G						1		5	10
		J								3	11

Table X gives only the number of cases. A comparison of the right and wrong cases, the subjective reactions and the failures is more clearly presented in Table XI. This table shows that a lower degree and a greater scope of attention were uniformly, in the case of all observers, induced by the Double Task Method. F's introspective estimations cover nearly the entire attentive scale, though there is nevertheless a marked tendency toward the higher degrees. This distribution agrees closely with his introspections, for he reports that the clearness of the auditory processes rises when a change is perceived; as a change is perceived, the processes immediately become clearer, the judgment of kind and rate is made, and then the auditory stimulus again lapses into obscurity.

G likewise covers the entire scale. Her results are very regular, and show a tendency towards the central part of the scale. J's estimations, unlike those of the other observers, are limited to the lower degrees of clearness. His attention upon the secondary tasks was maximal; and his work in them was, throughout, of the highest order.

TABLE XI

NUMBER OF CASES GROUPED ACCORDING AS THE CHANGE IS OF PITCH OR INTENSITY AND IS RAPID OR SLOW, WITH SUMMARY

Kind or Rate	Report	O.	Clearness 10-9	9-8	8-7	7-6	6-5	5-4	4-3	3-2	2-0
PITCH	Right	F	8	31	23	10	4				
		G		2	2	23	16	6	1		
		J						19	36	23	2
	Wrong	F		1	4					2	
		G				2	8	10	3	1	
		J							5	8	2
	Sub-jective	F									
		G									
		J									1
	No-reaction	F									7
		G						1	3	3	12
		J								2	6
INTENSITY	Right	F	6	25	21	6	1				
		G		1	7	3	2	3	1	3	
		J						11	18	28	1
	Wrong	F	1	3	3	1	1				
		G				3	11	9	5	2	
		J							1	1	1
	Sub-jective	F	1	1	1	2					
		G									1
		J							1	3	
	No-reaction	F				1			1	5	28
		G						3	4	12	23
		J								5	27
SLOW	Right	F	5	17	15	2					
		G		2	1	2	1	2	1		
		J						9	18	14	
	Wrong	F	1	15	10	3	4			2	
		G			3	6	20	16	3	5	
		J						2	11	17	4
	Sub-jective	F		1		1					
		G									1
		J							1	2	1
	No-reaction	F				1			1	4	21
		G						3	6	8	20
		J								2	20

TABLE XI—*Continued*

Kind or Rate	Report	O.	Clearness 10-9	9-8	8-7	7-6	6-5	5-4	4-3	3-2	2-0
RAPID	Right	F	9	22	11	4					
		G		1	4	22	13	7	2		
		J						15	19	9	2
	Wrong	F		6	15	8	2				
		G			1	1	3	3	4	1	
		J						4	12	20	
	Subjective	F	1		1	1					
		G									
		J								1	
	No-reaction	F								1	14
		G						1	1	7	15
		J								5	13
SUMMARY	Right	F	28	95	70	22	5				
		G		6	14	50	32	18	5	3	
		J						54	91	74	5
	Wrong	F	2	25	32	12	7			4	
		G			4	12	42	38	15	9	
		J						6	31	46	7
	Subjective	F	2	2	2	4					
		G									2
		J							2	6	2
	No-reaction	F				2			2	10	70
		G						8	14	30	70
		J								14	66

Table XI shows further that the greatest number of right cases occurs in the higher degrees, and conversely that the greatest number of wrong cases occurs in the lower degrees of clearness. This relation between the right and wrong reports is more clearly shown in Table XII, in which the reports are weighted.[40] The same system of weights is here employed as in the Single Task Method. A single addition is, however, made: half a point is added to the right cases for all reports grouped under *Fact of Change.*

[40] We again call attention to the "subjective" reactions. Out of 1160 cases reported under this method only 22 (about 2%) were "subjective."

TABLE XII

WEIGHTED SUMMARIES

O.	Report	CLEARNESS								
		10–9	9–8	8–7	7–6	6–5	5–4	4–3	3–2	2–0
F	Right	28.5	95.5	71.0	22.0	5.0				
	Wrong	4.0	27.0	35.0	18.5	7.0		2.5	16.5	87.5
G	Right		6.0	14.0	50.5	33.0	20.0	6.0	3.0	
	Wrong			4.0	12.0	42.0	48.0	32.5	46.5	89.5
J	Right						54.0	92.0	74.5	5.0
	Wrong						6.0	33.0	69.5	91.5

The crests of the curves of right judgments now fall with F, 7 places; with G, 5 places; and with J, 3 places, respectively, to the left of the crests of the curves of wrong judgments. The crests of the latter curves lie uniformly, for all observers, in the lowest degree of clearness, from 0 to 20. This result, as is shown by the preceding tables, is due to the great number of changes which were unobserved and during which the tone of the variator was very obscure. The general relation holds, however, even if the no-reactions are not considered: in that case, the crests of the curves of wrong reactions still fall 1, 2, and 1 place for F, G, and J respectively below the crests of the curves of right reactions. The results thus substantiate those of the Single Task Method, and confirm our conclusion that a close parallelism exists between the introspectively distinguished variations of attention and the accuracy of the work done,—provided, first, that the estimation of the degrees of attention is made in terms of the attributive clearness of the processes attended to; and, secondly, that the work itself is not influenced by anything other than a change in the attention. That this second requirement is fulfilled under the conditions of our experiment, and that our results are due to attention and not to any other influence, such as a specific reaction to a particular kind and rate of change, is not to be doubted. It is clearly shown in Table XI that the relation holds irrespectively of the kind and rate of the change. The crests of the "right" curves lie from one to two, three and four steps above the crests of the "wrong" curves.

It might be objected that the precision of the work is no more dependent upon degree of attention than upon duration of attention. That the *moment* of change is significant, an objector might say, cannot be questioned. Sometimes a change will occur when the addition is easy, or a cork has just been deposited within the box. Under such conditions a change may be freely attended to, and consequently both the degree and the duration of attention will be increased. If, on the other hand, a change occurs at some critical point in the counting or adding, though it may be perceived as clearly as in the former case, yet the duration of attention will necessarily be shorter; and consequently the precision of the work (judgment of kind and rate of change) will be decreased. In other words, the accuracy of the report will vary directly with duration of attention. This, indeed, might be a formidable criticism if the facts were found to justify it; but they are not. We might, in any event, raise the question whether attention, under the unfavorable conditions of the second case given above, could be as high as under the favorable conditions of the first. We might also point out that our observers were instructed to estimate the *clearness-values only*, that a long preliminary and practice period was allowed in order that they might accurately estimate the attributive clearness of mental processes attended to and attended from, and lastly that their introspective estimation of clearness-values was entirely independent of duration of attention. But we need only refer to the results of the Single Task Method. There, the observers were directed to attend *to* the tone, and *from* the distractors. They were not instructed to react as quickly as possible, but only to respond "as soon as a change is perceived;" there was no pressure upon them to hasten their report.[41] If the duration of attention varied at all, it varied quite independently of, and apart from, the clearness of the mental processes during the change, and the accuracy of the reports made after the change. As now the results of the two methods agree very closely, we may conclude without hesitation that degree and not duration of attention is responsible in our experiments for precision of work done.

It is apparent, however, from the preceding tables, that there is a difference in the *accuracy of reaction to the various changes*. The relation between report and objective change in the Double Task Method is shown in Table XIII.

This table shows, first, that a change of pitch is more compelling than a change of intensity; and, secondly, that a rapid change is more attractive than a slow. The results are uniform for all observers. With changes of pitch, more reports were correct, fewer were wrong, fewer subjective errors were made, and fewer objective changes were unobserved, than in the case of intensity; and likewise, though not so markedly, more rapid changes were reported correctly, fewer wrongly, fewer subjective errors were made, and fewer

[41] The phrase "as soon as" was not felt as a temporal pressure; for, as we have said, no one of the observers knew that the "reactions" were being measured. Had they possessed this knowledge, the instructions might have received a temporal interpretation. In fact, their only concern was to report correctly.

rapid changes were passed over unobserved, than was the case with slow changes.

TABLE XIII

RELATION BETWEEN THE OBSERVER'S REPORT AND THE KIND AND RATE OF OBJECTIVE CHANGE, EXPRESSED IN NUMBER OF CASES

O.	KIND								RATE							
	Pitch				Intensity				Slow				Rapid			
	R.	W.	S.	N.	R.	W.	S.	N.	R.	W.	S.	N.	R.	W.	S.	N.
F	76	7		7	59	9	5	35	39	35	2	27	46	31	3	15
G	50	24		19	20	30	1	42	9	53	1	37	49	13		24
J	80	15	1	8	58	3	4	32	41	34	4	22	45	36	1	18
Total	206	46	1	34	137	42	10	109	89	122	7	86	140	80	4	57

R., right report. S., subjective report.
W., wrong report. N., no report.

(1) The interval between the moment of change and the observer's reaction was measured, as before, in fifths of a second. The *average reaction-time* was computed for each degree of attention as estimated by the clearness of mental processes. This time, together with the m. v. and the number of cases at each level, appears in Table XIV.

TABLE XIV

AVERAGE REACTION-TIME IN SECONDS, MEAN VARIATION, AND NUMBER OF CASES AT THE DIFFERENT LEVELS OF ATTENTION

O.		CLEARNESS								
		10–9	9–8	8–7	7–6	6–5	5–4	4–3	3–2	2–0
F	Av.	1.13	1.22	1.46	1.30	2.10			2.30	
	m.v.	.48	.50	.66	.45	1.36			.30	
	No.	13	54	44	13	6			2	
G	Av.		1.30	.95	1.83	2.17	4.00	4.00	5.80	
	m.v.		.30	.27	.73	.77	1.30	.60	.13	
	No.		3	7	23	18	3	2	3	
J	Av.						1.10	1.32	1.61	1.10
	m.v.						.52	.63	.87	.20
	No.						24	46	48	4

Av., average reaction-time. m.v., mean variation. No., number of cases under rubric.

The number of cases in this table does not agree with that shown in Tables X and XI, although the table includes every reaction-time

at our disposal. The difference is due to three causes. First, the subjective reactions and the failures gave no reaction-times. Secondly, the delicate writing-point of the Jacquet chronometer not infrequently bound on the smoked drum. Since the apparatus was in another room, we had no intimation of this defect until the experiment was ended. Though such cases could be (and were) used when we were concerned only with a correlation between the introspective variation of attention and the accuracy of the reports, they were useless for the correlation between introspective variation of attention and rate of report. And thirdly, the observers under the Double Task Method did not always react as soon as the change was perceived and judgment made. They were not instructed to react immediately; and if a change occurred at a critical point, they might voluntarily delay their reaction until a more favorable opportunity to react presented itself. Such delayed reactions were noted in the observer's introspections, and are naturally omitted from the table.

The table itself corroborates Table V; there is a positive correlation between variation of attention and rate of reaction. The reaction-times in the higher degrees of clearness are shorter, and the mean variations are smaller, than in the lower degrees. The numerical expression of this correlation (for length of reaction-time) and of its probable error is as follows:

O.	Correlation [42]	P.E.
F	0.88	0.068
G	0.59	0.253
J	0.60	0.248

(2) Table XI shows that there is a positive correlation between variation in attention and accuracy of work performed *irrespectively of the kind and rate of the objective change.* Nevertheless, we have seen that changes of pitch, and rapid changes, are more compelling

TABLE XV

RELATION BETWEEN THE REACTION-TIME IN SECONDS AND THE KIND AND RATE OF THE OBJECTIVE CHANGE

O.	KIND						RATE					
	Pitch			Intensity			Slow			Rapid		
	Av.	m.v.	No.	Av.	m.v.	No.	Av.	m.v.	No.	Av.	m.v.	No.
F	1.29	.61	71	1.52	.65	48	1.78	.98	58	.99	.34	61
G	1.91	.75	42	2.28	1.40	15	2.73	1.01	18	1.68	.76	39
J	1.24	.53	73	1.54	.85	48	1.93	.82	49	.96	.28	72

Av., average reaction-time. m.v., mean variation. No., number of cases.

[42] In computing this correlation we omitted the data for F under the 8th rubric, for G under the 2d, 6th, 7th, and 8th rubrics, and for J under the last rubric. If these data had been considered in the computation the correlation would have been much greater. They are, however, the averages of too few cases: 2, 3, 3, 2, 3, and 4, respectively.

then changes of intensity or slow changes (see Table XIII). In Table XV, in which the relation between the average reaction-time of the observers' reports and the kind and rate of the objective change appears, this result is further confirmed.

The average reaction-times and the mean variations are uniformly, for all observers, smaller for pitch than for intensity, and smaller for rapid changes than for slow. These results agree with those of the Single Task Method.

(3) Table XIII shows the greatest number of right cases under the rubrics *pitch* and *rapid*, and conversely the greatest number of wrong cases under the rubrics *intensity* and *slow*. Since Table XV shows that the former rubrics have a more rapid reaction than the latter, it would appear that the *reaction-time for the right answers was faster* than that for the wrong. This inference is borne out by Table XVI.

TABLE XVI

THE REACTION-TIME IN SECONDS AND THE MEAN VARIATION OF THE RIGHT AND THE WRONG REPORTS

Observer	RIGHT			WRONG		
	Av.	No.	m.v.	Av.	No.	m.v.
F	1.26	63	0.52	1.51	56	0.79
G	1.56	29	0.70	2.45	28	1.46
J	1.26	59	0.59	1.46	62	0.73

Av., average reaction-time. m.v., mean variation. No., number of cases.

(4) The *effect of the secondary tasks upon attention* is shown in Table XVII. The average clearness of the sounds of the variator, the mean variation, and the number of cases for each task are given.

This table shows the effect of the division of attention. The fact that F and J have less changes under 0 than under 1 and 2 is due to the nature of our procedure. In some series, only one change occurred during the period of observation; and since the order was haphazard, these series might fall to one kind of experiment more frequently than to another. The difference, however, is small.

The average clearness of the auditory processes in the normal experiments is, for all observers, uniformly higher, and the mean variation is correspondingly lower than in the Double Task experiments. The secondary tasks are, on the average, very effective, though the large mean variation shows that there were cases in which the auditory processes were normally clear. This result is borne out by the introspective reports of the observers. There were many cases in which the change compelled the attention, so that the auditory process momentarily became maximally clear, and the task of counting or adding became obscure. The table shows, further, that the task of adding was uniformly more efficient than the task of counting. The larger size of the mean variation may be ascribed in the light of the introspective reports to the more frequent occurrence of "critical points" in the adding. It will be remembered that the rate at which the corks were counted was governed entirely by the observer, while the adding was objectively controlled.

TABLE XVII

THE AVERAGE CLEARNESS OF THE AUDITORY STIMULUS AS AFFECTED BY DIRECTION OF ATTENTION ON THE PRIMARY OR SECONDARY TASK

Observer		Attention directed to		
		0	1	2
F	Av.	88.5	70.3	53.4
	m.v.	4.5	13.1	29.5
	No.	87	98	100
G	Av.	85.0	48.4	35.1
	m.v.	3.1	12.5	17.7
	No.	93	92	94
J	Av.	92.1	25.5	21.8
	m.v.	2.8	7.3	8.9
	No.	82	100	101

Av., average clearness of tone. m.v., mean variation. No., number of cases under rubric. 0, attention directed to tone of variator. 1, attention directed to counting of corks. 2, attention directed to adding of figures.

TABLE XVIII

COMPARISON BETWEEN THE AVERAGE CLEARNESS OF THE MENTAL PROCESSES INVOLVED IN COUNTING CORKS AND THE DEGREE AND ACCURACY OF THE WORK PERFORMED

Observer		CLEARNESS OF MENTAL PROCESSES INVOLVED IN COUNTING CORKS			
		100–90	90–80	80–70	70–60
F	Av.	44.0	36.0	34.2	27.0
	m.v.	5.1	3.2	2.2	0.0
	W.	4	8	3	1
	No.	16	21	8	1
G	Av.	32.6	28.6	23.5	21.0
	m.v.	1.7	2.7	2.8	0.0
	W.		6	2	1
	No.	9	33	4	1
J	Av.	65.0	61.3	58.4	
	m.v.	3.5	3.5	2.8	
	W.	6	9	2	
	No.	17	29	5	

Av., average number of corks counted. m.v., mean variation. W., number of experiments in which the corks were incorrectly counted. No., number of experiments.

(5) Tables XVIII and XIX show the *relation between the character and quality of the work performed, and the attention* as estimated in terms of attributive clearness. Since the tasks occupied the entire period of observation, the clearness of the processes involved in them was obtained as the average of the period.[48]

F and G it should be remarked, counted the corks singly; J counted them in pairs.

The results under Av. and W. confirm our previous conclusions. When the mean variation is considered, however, a discrepancy appears. F and J show a greater m. v. for the higher degrees of attention than for the lower; G alone follows the rule. We can only suggest that the irregularity is due to the small number of cases.

TABLE XIX

COMPARISON BETWEEN THE AVERAGE CLEARNESS OF THE MENTAL PROCESSES INVOLVED IN CONTINUOUS ADDING AND THE ACCURACY OF THE WORK PERFORMED

Observer	Report	CLEARNESS OF MENTAL PROCESSES INVOLVED IN CONTINUOUS ADDING			
		100–90	90–80	80–70	70–60
F	Correct	20	6	1	
	Nearly correct	14	3	1	
	Failures		4	2	
	No. cases	34	13	4	
G	Correct	9	12		
	Nearly correct		18	2	
	Failures		4	4	1
	No. cases	9	34	6	1
J	Correct	13	5	2	
	Nearly correct	5	6		
	Failures	6	11	4	
	No. cases	24	22	6	

In Table XIX the correct additions are grouped under the heading *Correct.* Those in which there was a mistake of one digit, as 164 for 165, or 219 for 229, are classified as *Nearly correct.* And lastly those cases in which the error was greater, or in which the observer lost count of the numbers, are brought together as *Failures.* The degree of attention is determined, as in the previous table, as the average clearness of the mental processes involved in the adding during the entire period of observation.

F and J give the results that we have learned to expect: G's showing

[48] This average, though not asked for in the instruction, was voluntarily estimated introspectively by both F and G, and the value was taken that they ascribed. J gave no such estimation. His average was computed from his estimations prior to and between the moments of change. We may add that the same data were at hand (or the same plan was followed) for estimating the clearness of the mental processes at times when No Reaction was made to an objective change (see Tables I and X).

is less regular. During maximal attention, however, all of her additions were reported correctly. In absolute number, it is true, more correct answers were returned in the next lower degree of attention; but at this level we also find not only 18 cases in which the answer contained a slight error, but also 4 cases of failure. In view of the small number of cases, we may be fully satisfied with the general confirmation of our earlier experiments.

(6) The large number of introspective reports obtained during these experiments, nearly 1400 in all, should yield information concerning the *number of levels* of attention. Titchener, in his *Psychology of Feeling and Attention*,[44] reviews and discusses the literature, and concludes that "a diagram of consciousness would show . . . a two-level formation. The surfaces are not smooth; the upper certainly, the lower probably, is creased or wrinkled." These creases or wrinkles correspond to the finer differences of attributive clearness at the two main levels of consciousness. Since this review, two important contributions to the subject have been made. Wirth[45] finds, under certain conditions, that there are more than two levels of clearness in his own consciousness, and maintains that at any moment of attention all possible degrees of apperception may be represented. Geissler suggests that there are two more or less distinct types of observers: those for whom the dual division is the most natural and most common; and those who as a rule experienee several levels of clearness.[46] Only two of his observers reported a multi-level formation, and he was therefore very cautious in drawing his conclusions. "There seem," he says, "to be two types of the attentive consciousness, the dual division and the multi-level formation." Later, in a review and criticism of Wirth's results, he expresses the opinion that Wirth, and probably also Wundt, belong to the latter type.[47]

Our own results throughout corroborate Titchener's original conclusions.[48] All of our observers reported, without exception, the dual division: a clear focus and a vague background, which varied reciprocally.[49] In not a single instance was the multi-level formation even hinted. The distinction of the two types is, then, apparently, a true individual difference, and is not dependent upon external conditions.

The records thus show to date three observers of the multi-level type: Prof. W. Wirth,[50] Prof. M. Bentley,[51] and Dr. H. M. Clark;[52]

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[44] *Op. cit.*, 220-242.
[45] W. Wirth, *Phil. Stud.*, XX, 1902, 493 f.
[46] *Op. cit.*, 528 f.; *cf.* Titchener, *Text-Book*, 1911, 302.
[47] This JOURNAL, XXI, 1910, 155.
[48] *Op. cit.*, 220-242; also *Text-Book*, 276 ff., 290.
[49] The values of the upper and lower levels of attention did not in every case total 100. The discrepancy, however, was never very great, varying only about 5 degrees on the one side or the other. There was some individual variation among the observers. F was ordinarily about 5 *per cent.* short in the total; he never, however, made an error greater than this. G's totals were on the average 5 *per cent.* too large; frequently they were correct; at times they were 10 *per cent.* too large; the value never fell below 100. J's results almost always totaled 100.
[50] *Cf. op. cit.*, IV, 1908, 139 f.; *Phil. Stud.*, XX, 1902, 493; and L. R. Geissler, this JOURNAL, XX, 1909, 120-130.
[51] Geissler, *op. cit.*, 527 f.
[52] *Ibid.*, 528.

and eight observers of the dual type: Prof. E. B. Titchener,[53] Prof. W. H. Pyle,[54] Dr. T. Okabe,[55] Dr. A. de Vries,[56] Prof. L. R. Geissler,[57] and our observers, Dr. W. S. Foster, Miss M. E. Goudge, and Mr. J. S. Johnston.

III. *Summary.*

The general results of our study may be summarized as follows:

(1) Attention may be measured introspectively in terms of attributive clearness. For introspectively distinguished variations of attention (*i. e.*, clearness) are closely paralleled by corresponding differences at the same level in accuracy of work performed, in rate of reaction, and in degree of precision as expressed by the m. v.

(2) Under our conditions, the time of reaction, as is shown throughout by the high coefficients of correlation, serves accurately to measure the attention.

(3) Changes of pitch and rapid changes are more compelling than either changes of intensity or slow changes. The accuracy of judgment for change of pitch and for rapid change is greater than that for change of intensity or for slow change; and, furthermore, the reaction-time to the former changes is smaller than that to the latter.

(4) A close correlation exists between accuracy and rate of report.

(5) Under our conditions, distraction, no matter how slight, tends to lessen the degree of attention.

(6) The difficulty of obtaining a graded series of distractors is very great. In our experience, the action of the distractors is not constant, but varies from day to day, and from observer to observer.

(7) There are two types of the attentive consciousness: the dual division, and the multi-level formation. These types represent true individual differences, and do not depend upon external conditions of observation.

(8) In the dual-division type of attention, the levels vary reciprocally.

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[53] E. B. Titchener, *Lectures on the Experimental Psychology of the Thought-processes*, 1909, Lect. I.; also *Psychology of Feeling and Attention*, 220.

[54] Geissler, *op. cit.*, 527.

[55] *Ibid.*, 527.

[56] *Ibid.*, 527.

[57] *Cf.* this JOURNAL, XXI, 1910, 154 f.

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[1] In compiling this list, the writer has tried to make a beginning toward a comprehensive bibliography of *rhythm*. In order to make the list ultimately as complete as possible, he hopes that those who are interested in the subject will call his attention to any additions or corrections to be made in the bibliography. As a rule, text-books and other systematic works are cited only when they offer original contributions, either by way of theory or by way of experimental investigation, to the general topic. Books and articles, whose titular reference carries only an implicit connection with the subject, are not cited unless they are quoted by some investigator in the field of *rhythm*.

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CLINICAL NOTES ON THE EMOTIONS AND THEIR RELATION TO THE MIND

By George Henry Taylor

In forming a judgment of the emotional factors in a child or a man, it is necessary that every manifestation of emotion should be noted by the observer. His method of examination must be as thorough as that of a trained physician examining a case of illness where doubt exists as to its cause, or of an alienist when determining the sanity or insanity of a patient. A spot diagnosis by a highly trained intelligence is frequently correct, but it is obviously liable to error. A candidate, when examined by the modified Williams' Lantern, may, through a mental lapse and not because of color blindness, name a color wrongly, but he usually at once corrects his error. He may name a red-green, or a green-red; he will not, in my experience, through a mental lapse, name a red-white, or a green-white. This evidence amounts to a suspicion of color blindness, but it would be unjustly used if a candidate were condemned by it without other evidence being considered. The type of person I refer to never repeats his mistake, and shows by the body of evidence in his answers that he is not color-blind. He is never, however, in my opinion, a person with a keen appreciation of color. A person may misname an acquaintance without confusing his identity. He is at least not as liable to misname his wife or his child or his occupation. In the same way a misnaming of red, white or green by an engine driver has a greater significance than the same mistake under similar conditions of examination would have if made by an uneducated candidate for employment.

It may here be mentioned that a mental condition which is the cause of an error, and the trail of a man's life is dotted with errors until it ends, varies little as to the effect, although the consequences which flow from such error may be widely different.

With the average man who lives by labor, a struggle for existence concentrates his mental processes upon the safeguarding of himself and the persons who enter into the

orbit of his emotions. An ordinary man or woman is in sympathy with the emotions common to his or her class, and will frequently resent an emotional appeal which is outside of his or her environment. Among the excitant causes of emotions concerned with the appeal of color, I include certain conditions of sound. The appeal of conduct, or moral sense, I believe to be a recognition, in my mind, of the social obligations under which we exist, and the ability to restrain the appeal of emotions when they are in conflict with this recognition. As "the old order changeth, yielding place to new," what may constitute an active emotion in one phase of development, may in another phase pass entirely out of the emotional orbit of an intelligent and educated person.

There is in certain persons, consciously, and in others, unconsciously, a degree of imitation in emotional appeals. The mummer's attitude is largely present in ordinary life. The majority of men keenly resent a finding which exhibits a defect in one or more of their special senses, and as a consequence, a number of educated persons fail to recognise a well defined defect in themselves. I have conversed with a man who professed to a keen delight in color, and found him by examination to be a red-green blind. I have also lived with a person carefully trained in music to interpret the composition of certain composers on a piano, but who in a harsh voice would sing out of tune, and exhibit uniformly in expression and conduct a real indifference to the emotions of color and music.

In the evolution of emotion there is in the mind a dream condition when emotions at rest or in slight vibration become active. They appeal to the mind for recognition, and finally for expression. In rare beings there are periods in which these conditions are fully met. When the appeal of an emotion can survive criticism, it is gradually absorbed by the mind, and may in time, and under certain conditions, cease to vibrate. Without this preliminary stage of vibration, the conception of a newly born truth is incomplete. A purely scientific process can investigate and criticise, it can take to pieces with infinite care and precision, but it is prone to resent a truth if presented in the vibratory stage of an emotion. The mind seeks a "dry light" and the exclusion of emotion, and may to this extent be prejudiced and narrow in its view.

The stimulus an emotion can produce upon the mind, varies in its intensity at different periods. Every emotional

person will recognise this varying condition in himself,—how at one time the mind is in sympathy with an appeal, and at another is comparatively indifferent, or even resentful. The appeal of emotions to the mind may be divided into two degrees:—

(1) In which there is a preliminary stage of active vibration.

(2) In which the preliminary stage of vibration is slight.

The appeal of a truth in its vibratory stage is essentially an appeal to the young. The deep roots of conviction in a matured mind are rarely disturbed by such an appeal.

The inherited emotional factors in a human being are the emotions concerned in the protection of life, and the necessary association with this of the evolution of intelligence, on the one hand, and the emotions which have their origin in sex, on the other hand. If one of these is defective, then the fundamental emotional factors in the unit are incomplete. An evolution in the emotions which originate in the sense of sex, and among these I include the emotions of color and musical sounds, can only become coherent to a highly endowed and critical intelligence when such intelligence has itself reached an evolutionary stage, capable of interpreting such appeal.

In illustration of my attempt to give a definition of Moral Sense in the foregoing, I described the effect of education upon an average citizen. It should however, be noted, that in a percentage of persons the place of an appeal to the intelligence in regard to conduct is taken by a purely emotional condition. It is in this type that inheritance of color and of musical sounds is found. The emotional type is in marked contrast to the intellectual type I then described. The manifestation of moral sense through the emotions may find expression by different channels. Amongst the poor and uneducated in whom the moral sense generally finds its medium through emotion we have this type in its most perfect form. Music, color, and the emotional qualities which take the form of a keen sympathy with, and entrance into the pathos of life, is expressed in a type approximating Christ, the great exemplar of emotional morality. Emotional morality is in fact independent of any high intellectual development. Indeed a reference to the intellect in criticism of such emotional manifestations appears to be evidence in the unit, of a deficiency in the supreme sex appeal—a manifestation of absolute emotion—consequently what is termed

genius in a painter, a poet or a musician, is a condition in which an appeal to reason is of small value.

It is interesting to note the practical demonstration of this idea in the history of emotional interplay on the various priests of Christ. A sincere and highly intellectual priest tortures or kills persons whom he regards as a menace to his faith, whilst others of the priesthood in the same period regard such acts with abhorrence. If a person criticise these two types without prejudice, he must, I think, recognise on the one hand a reasoning man, and on the other an emotional being who, resenting cruelty, can only appeal to humanity through his own medium of emotion. On a somewhat similar plane is the modern social reformer who, without regard to the suffering and destruction he causes, would precipitate revolution to consummate reasoned idea. Contrasted with this is the man who, resenting violence and cruelty, strives through an appeal to humanity, that is through the stimulus of the emotions, to bring about the same result. The genius—offspring of emotion—is rarely found amongst those who have accumulated wealth. It is the appeal of the tragedy and pathos, the comedy and beauty of life which brings him into being and equips him with the power to love, and to express love to humanity through music and painting, conduct and teaching.

An analogy on these lines can be made between a sex dream, and a nightmare. One is a delirium of sex, the other a riot in the intelligence. There is no fear in the one, fear is the prominent phenomenon of the other. Fear is an intellectual appraisement, whilst love is the emotional manifestation of sex. These appear to be the conditions of a supreme intellectual and sex appeal—and "Love casteth out Fear." Under such a stimulus must Beethoven have composed his "Kreutzer Sonata" on the one hand, and Poe his "Raven" on the other; the one a purely emotional outpour, the other a piece of intellectual artistry.

The creative musician, the painter either in words or upon canvas, and that still rarer form of emotional type, the Christ-like man, all make their supreme appeal through emotion. It is, I believe, in the last analysis, a sex appeal, purged of animalism, and is nearly always an appeal from a man, and through men to women and children. The mind of man, taking mind in this sense to include both intellect and emotion, may for the purpose of demonstration be compared to a straight line. At one end of this potential line is reason,

at the other, the emotions having their origin in sex. The emotional and intellectual conditions of a commonplace man in a civilized community (and the large proportion of men are commonplace) differ in education and environment more than in intellectual or emotional inheritance. He stands within a limited orbit somewhere about the middle of this line. If we judge the value of men by an economic standard, then it is probable in this assessment that a hod-carrier is of greater value to the community in which he lives, than is an emotional genius. It is simply a question of standards. At the reasoning end of the straight line I have described, sex would, in such an economic standard, be regarded as a function with periods of excitement in the unit. The male mind, apart from lust, would then appraise the value of a female as a child producer and mother. This appraisement would differ in values though not in principle when made by a brute type on the one hand, and a high intelligence on the other. The appeal of moral sense in the ordinary man I have described, is very little more than a recognition of danger from the standpoint of law or social convention. If he recognise a danger signal he will frequently disregard it, provided he can do so with safety and advantage to himself.

A RAPID AND ACCURATE METHOD OF SCORING NONSENSE SYLLABLES AND WORDS[1]

By Darwin Oliver Lyon
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In all memory work, the determination of the subject's retentive capacity is undoubtedly the chief factor. When we determine retentiveness by the *time taken to relearn* after a certain interval has elapsed, the method is easy, since the only measurement we have to consider is *time*, i.e., we compare the subject's *time of relearning* with his *time of initial learning*. When, however, we desire to ascertain the subject's retentiveness, say after an interval of one week, by requesting a reproduction without a fresh presentation, the determination is more difficult since our only method then is to determine the subject's retentiveness by the amount and nature of the material that he has actually been able to reproduce, i.e., to recall.

Where logical or meaningful material (e.g., a passage of prose or poetry) is used, the scoring of the work reproduced offers fewer complications than does the scoring of an attempted reproduction of a set of nonsense syllables or words. The latter may not necessitate so much time; but the difficulty of dealing with the errors, omissions, insertions, etc., makes an accurate scoring exceeding difficult since it is difficult to determine, from the reproduction, what associations were originally formed, because the material is less logical in character.

In 1908 I started experiments with digits, words and nonsense syllables in which it was necessary to determine the amount retained by each subject after an interval of one week had elapsed. Digits are so simple in character that they offer but few difficulties; but although many methods were tried for nonsense syllables and words, none of the methods seemed satisfactory. The widely different marks obtained with the various methods of scoring in general use, show that some of these methods must be erroneous. For nonsense

[1] The method as set forth in this article refers only to words and nonsense syllables of three letters.

syllables, probably the most accurate of the methods tried was that of Ebbinghaus; but even here not only was the method long and laborious, but the scores obtained were frequently such that I felt quite sure, from a general survey of the attempted reproductions and from a comparison with the subject's retentiveness for other material, that the scores as given by the Ebbinghaus method were too low.

In the following pages I have attempted to describe a method which I have found to be most satisfactory for both nonsense syllables and words. Its chief advantage lies in the speed with which each subject's score can be obtained after one has become familiar with the method. Probably the best way of obtaining an understanding of this method is to examine a few typical cases.

At the end of this paper will be found copies of the reproductions as handed in by three subjects in a class in experimental psychology. The nonsense syllables and words are given just as they were written, with the errors, omissions, dashes, etc.

The original list of nonsense syllables and words was as follows:

VUS	TUB
YIF	PIN
MAV	HEN
JEP	BED
VOB	LID
FEG	GEM
WOF	BUD
TIB	CAR
NUZ	MAT
BOF	ROD
JED	JUG
KIB	FOG
VEL	LAD
ZID	SOD
BOL	PEN
SEF	CAT
YAB	RAG
KUV	BOX
TEF	NET
NAD	GUN

Briefly stated, the method for nonsense syllables was as follows: Each correct letter, provided the syllable was in the

correct position,[2] received a score of one, and the syllable received an extra score of one for being in the correct position. Thus a perfect syllable in the correct position received a mark of 4, while a syllable correct in itself, but not correct in position, received a score of only 3. If the position were correct and the syllable had two of the three letters correct[3] it was scored 3. If two of the three letters of the syllable were correct but the *position* of the syllable itself were not correct, either relative or absolute, it was not scored at all. Therefore, unless position is correct, the separate letters do not count unless *all* are correct.[4] It must be remembered that, as before said, if a syllable is correct, but is not in the correct position, it gets 3, and only 3, counts, since each syllable that is in the correct position and also correct in itself receives a count of 4. Therefore, the highest count obtainable for a list of 20 syllables would be 80. This method of scoring can be made clear by examining a typical case. Take, for example, the list written by J. McH., as given at the end of this paper. *VUS* gets 4 counts, since it is correct in everything. *VIF* gets only three counts, since, although its position is correct it starts with V instead of Y. *JEP* gets 3 counts; had it been in the correct position it would have received 4, since when a syllable is correct, except that it is in the wrong position, it is credited with 3 counts,—one for each letter. *RIL* receives no score at all, since there was no such syllable. *BOV* receives a score of 2, for it contains all the letters that occur in *VOB* and besides is in the correct position, i.e., where *VOB* should be. *SIR* receives no score at all. It is quite likely a pure guess, and put down merely to secure correctness of position for the two following syllables. We are led to believe this when we perceive that the next two syllables, *WOL* and *TID*, have two letters correct in each, and their positions are also correct.

The subjects were told to draw a line under the last syllable they wrote if they felt sure that it *was* the last syllable. In this way the last syllable was given a score of 4 if it was the correct syllable and was also underlined, even if it was

[2] "Correct position" here, as with digits, may mean correct *relative* position or correct *absolute* position. A syllable is in the correct *relative* position when it is preceded by the correct syllable, or by a syllable of which two letters are correct, provided that these letters themselves be in the right order.

[3] Provided these two letters themselves were in the correct order.

[4] When, however, all three letters were written but not in correct order, i.e., the letters reversed,—the syllable received a score of 1, and if the position was correct, a score of two.

not preceded by the correct syllable;—it was given a score of 4 since it had the correct absolute position.

With words a method similar to that used with nonsense syllables was employed.

A score of 1 was given if the position, whether relative or absolute, was correct. Here also correctness of the relative position was determined by the preceding word. An extra count was given if any two letters[5] were correct, provided that the position of the word was correct. If the word itself was correct it received still an extra count, thus raising the count to three. Therefore, the highest count obtainable for any word was 3, and therefore, the highest score obtainable for the 20 words was 60. This method, like that for the nonsense syllables, can best be understood by an examination of one of the subject's papers. Take, for example, that of the subject reproduced at the end of this article. Let us see how we arrived at the total score of 15. *TUB* received a score of 3 since it is in the correct position and the word itself is correct. *PEN* receives a score of 3, one because two letters of the word are correct, one because the word (as far as the two letters are concerned) is in the correct position, and still an extra mark being given since the word itself is one occurring in the list.[6] *CAT* receives a score of 3,—one because it contains two correct letters, 1 because it is the correct word itself and another count because it is in the correct position, being preceded by the word *PEN*. *MAN* receives no count at all, there being no such word. *PIN* receives a count or 2,—1 because it contains two correct letters and an extra count because it is the correct word. It cannot receive a count for position, neither the relative nor the absolute position being correct. *RUG* receives no count at all. We are tempted to give it a mark of some sort on account of its close similarity to the word *JUG* but this would be precarious as there are many words that rhyme with *JUG* and many such words might have been written at random. We are also tempted to give the word *RUG* a score of 1 as it is similar in meaning to the word *MAT*. This also would be unwise since many words might have been given,

[5] The same rule was used here as in the case of the nonsense syllables and the two letters themselves had to be in the correct order.

[6] This is an exceptional case. With the list in question it is the only word with which such a case can occur, for the reason that, though in one sense the word is not in the correct position, yet two letters of it are, it being in the place that *PIN* should be. Obviously, cases similar to this occur very rarely and would not occur at all if the list of words used did not contain two words so nearly alike as *PEN* and *PIN*.

that in the examiner's mind might have been considered similar to the word *MAT*. In fact in the case of this particular subject we have proof that the word was not put down for this reason as the word *MAT* itself is mentioned as the next word. Therefore, taking everything into consideration, I considered it safest not to give any credit whatsoever for such words. The next word *MAT* gets a score of 2,—1 because it has two letters correct and an extra 1 because it is the correct word itself. *RAG* gets a score of 2 for the same reason. The next four words, *WIT, RAT, BOY* and *RUN,* receive no credit whatsoever, there being no such words.

A score of 1 was not given a word having two letters correct unless the *position* of the word itself was correct. Otherwise the word *RUG* would receive 1 count since it contains the letters *UG*. Had *RUG* been preceded by the word *ROD* it would then have received 2 counts instead of none at all,—1 count for having two letters correct, and an extra count for being in the correct position.

To make this method of scoring still plainer, we shall examine another paper,—that of subject M. K., also reproduced at the end of this article. The first word *TUB* is given 3 counts, it having 2 letters correct, it also being the correct word, and also being in the correct position. *HEN* is given a score of 2, it being the correct word but not in the correct position. For like reasons *JUG* is scored 2. *RAT* receives no score at all although it has two letters, *AT,* that are correct (they being also in the word *CAT*). The word, however, is not in the proper position either relative or absolute and hence can receive no count at all. Words of this kind receive a score of 2 or nothing, for reasons given in detail under nonsense syllables. The fairness of this rule is made clear when we realize that had the word *RAT* been preceded by the word *PEN,* the chances of *RAT* having been a mere guess would be greatly lessened. *TAN* receives no count at all. To the next word, *MUG,* one is tempted to give a score of 1, since it contains the two letters *UG* which are also contained in *JUG*. It would have received credit for these two letters had the word been preceded by *ROD*. Not being preceded by *ROD* it is given no count at all. That this is perfectly fair is in this particular case very conveniently shown by the appearance later on of the word *RUG* which, although there is no such word, is given a score of 2, it being preceded by the correct word *CAT*. The two letters that are correct in this case are *R-G* and although separated by the wrong vowel *U* they are in the proper order. *PEN* receives a score of 2, it having two letters correct and also

being the correct word itself. *BED* receives a score of 3, 1 because it contains two correct letters, 1 because it is the correct word itself, and 1 because it is preceded by the correct word. In this case the "preceding" word is not wholly correct but it contains two correct letters, and thus *BED* gets a higher scoring than it would have received had it been preceded by the word *AXE*, for example. The last word *GUN* receives a score of 3, it being in the correct absolute position for the reason that it is underlined, this proving that the subject knew that it was the last word.

Reproduction of Nonsense Syllables and Words
By J. McH.

VUS....	TUB...
VIF...	PILL..
JEP...	RAG..
RIL	CAN
BOV..	———
SIR	———
WOL...	———
TID...	BAR..[1]
	DOG
	SUN
Total Score for	FLY
Nonsense Syllables	———
18	———
	MAT..
	BAG
	BOX...
	TOP
	LID..
	MAN

Total Score for Words, 16.

Reproduction of Nonsense Syllables and Words
By A. F.

VUS....	TUB...
YIF....	PEN...
MAV....	CAT...

[1] *BAR* receives a score of two although it is neither preceded by the correct word nor is the word itself correct. The correct word here is *CAR*,—and *BAR* receives a score of 1 for having two letters correct and another score of 1 because the word is in the correct absolute position.

JEB...	MAN
VOS...	PEN..
WEF...	RUG
FEG...	MAT..
TIB....	RAG..
NUZ....	WIT
LOD..[8]	RAT
GER..[8]	BOY
KUL	RUN
YAB...	
Total Score for Nonsense Syllables 39	Total Score for Words 15.

Reproduction of Nonsense Syllables and Words By M. K.

VUS....	TUB...
YIF....	HEN..
TIB...	JUG..
BIF[9]	RAT
JEB...	TAN
———	MUG
———	CAT..
———	RUG..
NAB...	PEN..
———	BED...
	GUN...
	———
Total Score for Nonsense Syllables 17	Total Score for Words 19.

[8] *LOD* and *GER* both receive a score of 2 notwithstanding that they have only one letter correct, i.e., the vowel. As said on page 527, even though a syllable has two letters correct, if it be not in the correct position (either relative or absolute) it receives no score at all. When, however, the *absolute* position is correct (as it is in the above case) *each letter that is correct is scored.* Therefore, each of the above two syllables receives a score of 2,—1 because it is in the correct absolute position, and 1 for having a correct letter.

[9] *BIF*, though similar to the tenth syllable *BOF*, can receive no score, it being in neither the correct *relative* nor the correct *absolute* position. It has, however, two letters that occur in *BOF*, for which it may have been mistaken. *JEB* (*JED*) on the other hand receives three counts, it being in the correct relative position with reference to the preceding syllable *BIF* (*BOF*). It will thus be seen that in cases like this, what one syllable loses it gives to the other.

CHARACTERISTIC DIFFERENCES BETWEEN RECALL AND RECOGNITION

By H. L. Hollingworth
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The more obvious practical importance of *recall* in daily life seems to have led the greater part of experimental work on memory in this direction, to the comparative neglect of the not less interesting process of *recognition*. The recent work of Müller, Strong and others has called attention to certain instructive and hitherto unreported differences between the two processes. Thus Müller reports that retroactive inhibition fails to appear if partially learned material, followed by some different task, is merely recognised. But in the case of reproductive memory the addition of the incidental task is found to bring about a considerable reduction of recall efficiency. Strong finds that whereas, in recall memory " with increase in the length of the series there is much greater corresponding increase in the time or energy required for its mastery," in recognition memory the results indicate " that the number of stimuli in the series affects the results almost in direct proportion to the increase." Kirkpatrick finds that, under given circumstances, about twice as much can be recognised as can be recalled.

It is probable that a more careful study of the phenomena and laws of recognition will throw light on various other processes concerning which there is still much to be learned. Feelings of identity and of familiarity are fundamental in many intellectual operations, so fundamental indeed that one is tempted to classify them as ultimate experiences which cannot be further analysed. Since this short paper is intended to be suggestive rather than final, no attempt will be made to summarize previous work on recognition. The results to be reported were secured, for the most part, in experiments performed for other primary purposes. They are thus, in most cases, incidental results, but are perhaps for that very reason even more suggestive than if they had been deliberately secured from the point of view in which they are now considered.

Schematically, at any rate, the difference between recall

and recognition seems to be a rather simple matter. Recall is that aspect of memory process in which a *setting,* a background or association-cluster, is present in clear consciousness, but a desired *focal element* is missing. Thus I try to recall the name of Byron's hero in "The Prisoner of Chillon." The memory of the theme, the aspect of the castle, the pillar in the dungeon, the beauty of the lake and mountains, constitute a clear setting, but the focal element, the name, is missing. When it appears it probably comes in fragments,—first the form or rhythm of the word, then various letters or syllables, one fragment dragging in the others and being assisted in this by various features of the general setting. The following series, resulting from an actual attempt to recall this name, shows the way in which the unitary focal element is finally constituted by its various fragments:

*B*alboa
*Bom*b*ard*o
L*omb*roso
Bazzadof
*Barbad*oes
L*ombard*o
*Bonav*entura
*Bonav*ent
Bonivar
Bolivar
Bonivard (correct)

Recognition is, schematically, just the reverse of this process. In recognition the focal element is present, in the form of sensation, image, or feeling, and the question is whether or not this element will recall a more or less definite general setting or background. Such experiences as those indicated by, "Where have I seen that face before?" "Have I ever heard this sermon?" "Whose voice is that?" etc., illustrate this situation. It is indeed often true that the setting need not come into clear consciousness in order to determine the outcome of judgment or the feeling of recognition. At least it need not become as clear as the recalled element in reproductive memory, in order to produce the feeling of familiarity. But the mere feeling of familiarity represents only a partially complete recognition. The completeness of the recognition will depend on the clearness or briskness with which the setting or certain features of the setting happen to be revived. James has vividly described

the way in which vague marginally revived processes may reveal their presence, character and behavior by the production of a resultant feeling (familiarity, recognition, novelty, intention, etc.).

It is often said that recognition is an important part of recall memory, giving warrant to the correctness of the recalled element. This may often be the case, but it is not necessarily so. Items may be correctly recalled but not recognised as correct and rejected. Moreover every case of recognition presumably involves recall or tendency to recall on the part of the setting. The ordinary act of "memory" is said to be complete when focal element and setting belong together, that is, mutually recall or sustain each other.

If this schematic distinction between setting-element and element-setting is a correct one, it at once becomes interesting to picture some sort of neural counterpart of the two processes; to conceive in neural terms the differences in the behavior of the two aspects of memory. Why, for example, does retroactive inhibition affect the one process and not the other? Why does increase in series length influence the two processes in different degrees? Why is more material recognizable than can be recalled? Nor are these the only questions to be raised, for there are many other empirical differences between recall and recognition. Some of these further differences it seems worth while pointing out, even though the data on which the comparisons are based are so incomplete as to be suggestive only of qualitative rather than of quantitative differences.

1—Determination to Remember

As is well known, mere repetition, without the purpose or intention to retain, does not by any means guarantee the subsequent ability to recall. In fact the determination to learn is one of the most important conditions of reproductive memory. Rather curiously the influence of purpose or intention in the case of recognition seems to be much less than in the case of recall. The results of the following experiment suggest the striking difference between the two cases.

In an experiment performed primarily for another purpose than the study of memory, each of five observers went through a form of the "opposites" test from 60 to 75 times. The stimulus card bore 50 adjectives and the task was to speak the opposite of each word in turn, as quickly as possible. One or two trials were made each day, the same

list being used but the words occurring each time in a new and random order.

After 60 to 75 trials had been made, each observer was asked to recall and write down all the pairs of opposites that had been used. Fifteen minutes were allowed for this test, but the number recalled after the first three minutes was also noted.

After this test a list of 100 pairs of opposites, containing 50 new pairs along with the original 50, was presented. Each observer was now requested to identify the 50 pairs used in the experiment. In this case all observers completed their selection in three minutes or less.

The following Table gives the number of correct recollections and recognitions for each observer, and the averages of the five observers. Only slightly more than half (28.8) of the 50 items were recalled in a quarter of an hour, about half of these being reproduced in the first three minutes. In the case of the recognition test, however, practically every item is correctly identified within three minutes or less.

TABLE 1

INFLUENCE OF INTENTION ON RECALL AND RECOGNITION

Observer	From a total of 50 original items		
	Number recalled		Number recognized in 3 min. or less
	In 3 min.	In 15 min.	
L	19	38	50
G	14	27	49
H	10	27	49
R	12	25	50
P	19	27	50
Averages	14.8	28.8	49.6

Here we have a case of purely incidental memory, there having been no determination whatever to memorize the list of stimulus words. As a result, no doubt, of this absence of intention, the 60 to 75 repetitions failed to make sufficient impression to make possible more than 30 per cent recall efficiency. But in spite of the absence of intention to remember there is practically 100 per cent recognition efficiency, and the indications are that this efficiency would have been found

long before the last repetition. It is not possible, on the basis of these figures, to say that recognition is not influenced by the presence or absence of the determination to remember, but it is clear that it is much less influenced by this factor than is the process of recall.

2.—Value of Repetitions

The above result suggests that a given number of repetitions of the material has greater value for recognition than for recall. It is, of course, a matter of common experience that a single presentation may suffice to enable recognition but be quite insufficient to make recall possible. It would be of interest to determine more or less precisely the equivalence of repetitions in the two cases, for different materials and observers. The following results are suggestive of the sort of differences revealed by experiments designed for this purpose.*

Fifteen items were presented visually, at intervals of two seconds, to each of five observers. Each observer, immediately after the completion of the series, was requested first, to recall and reproduce or describe as many as possible of the fifteen items, and then to select, from a set of thirty items, the fifteen just presented. This method thus gives an approximate measure of the tendency to perfect recall on the one hand and the tendency to perfect recognition on the other hand, resulting from the single presentation. The series was then presented a second time and the recall and recognition test repeated. This process was continued until both perfect recall and perfect recognition were achieved, and the number of repetitions required in the two cases was thus determined. Four different sorts of material were used,—nonsense syllables, simple geometrical forms, pictures, and words (nouns and adjectives). Table 2 gives, for each observer and for each sort of material, the number of repetitions required for perfect recall and for perfect recognition, and the ratio of recall to recognition.

The results are rather interesting. When the material is quite devoid of sense or meaning, as in nonsense syllables, there is very little difference between the number of repetitions required for complete recall and the number necessary for complete recognition. As we pass from nonsense syllables through geometrical forms and simple pictures to common nouns and adjectives, the sense or meaning of the material becomes more and more definite and the possibilities

* These experiments were performed by Miss Edith Mulhall, Barnard, '13.

of associative setting increasingly richer. As this happens we find a correspondingly greater difference in the effect of repetitions for recall and recognition. The recall-recognition ratios increase from 1.2 through 1.4 and 1.9 to 2.2 respectively. This change in the ratios is furthermore due entirely, in these instances, to the increasing ease of recognition in the case of meaningful material. The average number of repetitions required for complete immediate recall changes very little. In general then, with meaningless material repetitions are equally effective for recall and for recognition. But with increase in the meaningful character of the material this influence becomes relatively greater in the case of recognition, until, with nouns and adjectives, it is more than doubled.

TABLE 2

EQUIVALENCE OF REPETITIONS FOR RECALL AND RECOGNITION

Material	Process	Repetitions for different Observers					Averages	Ratio of Recall to Recognition
Words....	Recall.....	3	3	5	3	6	4.0	
	Recognition....	1	1	2	2	3	1.8	2.2
Pictures..	Recall.....	3	3	3	2	4	3.0	
	Recognition....	2	1	2	1	2	1.6	1.9
Forms....	Recall.....	4	3	4	4	3	3.6	
	Recognition....	3	2	3	2	3	2.6	1.4
Syllables..	Recall.....	5	5	7	6	5	5.6	
	Recognition....	3	3	6	6	5	4.6	1.2

3.—Degree of Assurance

Watt remarks, "unfortunately, however, no very reliable test of recognition is known. A learner can either recall a word or he cannot. . . . But he may say he recognises a word without either being sure that he does so, or without really recognising it at all." And, "Correct and sure recall ought to bring with it the assurance of its correctness."

But does not recognition also bring with it the degree of assurance of its correctness? All that is necessary for a reliable test of recognition is a situation in which the chances of accidental correctness are known. This situation being given, it is no difficult matter to test the accuracy of recognition for various individuals, materials, and conditions.

TABLE 3

CORRECTNESS AND ASSURANCE IN THE RECOGNITION OF DIFFERENT MATERIALS

SYLLABLES					GEOMETRICAL FORMS				
Obs.	A	B	C	D	Obs.	A	B	C	D
1	72	65	72	60	1	97	72	50	60
2	78	50	75	..	2	85	..	..	..
3	72	63	65	44	3	84	65	55	33
4	81	52	44	..	4	95	70	..	..
5	76	51	67	..	5	90	50	50	..
6	89	63	50	50	6	90	75	75	..
7	67	63	56	..	7	90	75	67	..
8	63	27	33	..	8	64	55	75	(100)
9	89	76	63	55	9	82	67	58	(100)
10	75	50	..	50	10	87	50	..	..

WORDS					PICTURES				
Obs.	A	B	C	D	Obs.	A	B	C	D
1	90	100	72	50	1	100	84	67	72
2	68	53	25	..	2	89	..	..	..
3	71	60	83	50	3	89	81	77	..
4	91	60	..	..	4	95	80	33	..
5	92	..	..	..	5	98	..	..	..
6	95	57	33	..	6	96	63	50	..
7	87	50	71	42	7	100	..	..	..
8	89	60	60	50	8	96	75	..	..
9	90	80	30	..	9	93	100	..	..
10	80	85	66	..	10	93	100	..	..

AVERAGES OF THE 10 OBSERVERS

Material	A	B	C	D
Syllables	76.2	56.0	58.0	52.0
Forms	86.4	64.0	61.0	50.0
Words	85.3	67.0	55.0	48.0
Pictures	93.0	83.0	57.0	....
Grand Averages	85.2	67.5	57.8	50.0

A test of the accuracy of recall requires only simple enumeration of correct reproductions and their proper relative evaluation. A test of the fidelity of recognition requires more elaborate statistical treatment of the data, perhaps, but the two methods are not unequally reliable. Just what degrees

of assurance recognition does bring, and just how correctness varies with this confidence, has been but little investigated.

The figures on page 538 resulted from experiments on this point. Syllables, geometrical forms, advertisements (picture and reading matter), and words were used, and the attempt made to measure the curve of forgetting for recognition, by testing at various intervals after the original presentation. This method was soon seen to be inadequate for the primary purpose of the experiment, since at each test the original material was again seen (though along with other material, to be sure) and the various items chosen at a given trial reinforced by the mere fact of their having been thus selected. But from the point of view of assurance and correctness the experiment afforded ample material at each trial. Ten observers were used, and the degree of assurance indicated in the case of each selection by grading the confidence of the identification as A, B, C or D. The Table gives typical results, showing the total correctness of each degree of certainty for intervals covering a period of twenty-one days.

Several suggestions are afforded by the Table.

a.—The correctness for C and D degrees of assurance (slightly certain and mere guess) is about the same for all four kinds of material. The mere guesses show just the chance relationship (50%, since the items were selected from a larger group containing twice the original number), and the C judgments some 8% higher correctness.

b.—The A and B judgments have higher and higher validity as one passes from nonsense syllables through forms and words to more complicated and meaningful material (advertisements). With syllables, A and B judgments are about 66% correct, with forms and words about 75%, and with advertisements about 88%.

c.—In these latter cases the C and D judgments are less used than is the case with less meaningful material. The four kinds of material show a regular progression in the number of observers not using the C and D degrees of confidence. The A and B judgments are thus more often correct in spite of the fact that more of the items are reported with these high degrees of assurance.

Strong has studied recognition for advertisements and for words, and finds varying percentages of correctness for different degrees of confidence as the length of series is increased. The same investigator has also considered some of the statistical difficulties involved in the correct evaluation and scoring of data secured by the method of selection.

4.—Influence of Primacy and Recency

That primacy and recency of impression influence the accuracy of recognition in much the same way that they influence recall, is seen from the following experiment. Fifteen pages, each containing an illustration along with reading matter, were arranged serially on a table. Twenty observers were allowed to begin at one end of the series and inspect all the items, from left to right, for a short period of time. These fifteen items were later presented along with an equal number of new items, and each observer attempted to identify the original set. The following Table gives the per cent of times that the items in the fifteen different positions were identified.

TABLE 4

INFLUENCE OF PRIMACY AND RECENCY ON RECOGNITION

Position	Per cent. of times identified	Averages of groups of 5 positions
1	84	
2	36	
3	44	
4	36	
5	20	44
6	12	
7	16	
8	4	
9	8	
10	20	12
11	4	
12	20	
13	28	
14	16	
15	56	25

Quite as in the case of the reproduction of simple material after serial presentation, we find primacy and recency both effective, and the former, under the conditions just described, considerably more influential than the latter.

5.—Recognition Span

The fact that there is a more or less definite reproductive "memory span," which varies with the individual, with the material, and with other conditions, is familiar. Aside from

this elementary span, with perfect reproduction, only a limited number, from a larger number of presented items, can be reproduced, and this number varies not only with individual and with material, but also with such factors as series length, time interval, etc. Much the same thing holds for recognition, although but little evidence for it has been presented. Strong finds that, "When five advertisements are successively exposed 86% can be recognised immediately after, while only 47% can be recognised from 150 advertisements similarly exposed. The per cent of correct recognitions decreases as the length of the series increases," and "This decrease is possibly faster at first and then steadily becomes less as the series are increased in length." The following results also bear on this question of immediate "recognition span."

TABLE 5

Per cent. of 15 advertisements immediately recognized, average. of 25 observers	.78
Per cent of 25 normal advertisements, average of 20 observers	.76
Per cent. of 25 geometrical forms, average of 20 observers	.62
Per cent. of advertisements when original copy is retained but the cut is changed	.56
Per cent of advertisements when original cut is retained but the copy changed	.43
Per cent. of times substitution of a new cut is detected	.26
Per cent. of times substitution of new copy is detected	.17

The fidelity of recognition clearly varies with the type of material. It also depends on the integrity of the original items, and this fact seems to indicate that the recognition is not entirely of the item as a whole, but is conditioned by specific details in the total composition. Experimental variation should show in any given case just which details are most important.

6.—Recognition after Longer Intervals

Similar differences in fidelity of recognition for different sorts of material are found when a longer time intervenes between the original presentation and the occasion of identification. The following results are from an experiment on the curve of forgetting for recognition, and show also the decrease in per cent fidelity which results when the number of items in the original series is increased.

The nonsense syllables show a considerable loss, both from increase in series length and from increase in interval. Words show a smaller loss, although loss is present from both

TABLE 6

PER CENT. OF CORRECT RECOGNITIONS, WHEN CHANCES OF ACCIDENTAL CORRECTNESS ARE AS 1:1. AVERAGE OF 10 OBSERVERS

	Advertisements	Forms	Words	Syllables
	After 2 days			
Series of 15 items....	93	85	78	75
Series of 25 items....	94	75	74	60
	After 14 days			
Series of 15 items....	80	81	77	66
Series of 25 items....	80	80	72	56

causes. Forms show no loss due to increased interval, and only after the shorter interval (two days) is there loss due to increased series length. Advertisements, on the other hand, show no loss whatever as a result of increase in series length, but lose appreciably with increased interval.

7.—Individual Differences

Individual differences in fidelity of recognition are apparent in any experiment with this process. The range of these differences may be roughly indicated by the ratio of best to poorest in any given group of observers. Using college students, the range is usually about as follows.

TABLE 7

RANGE OF INDIVIDUAL DIFFERENCES IN FIDELITY OF RECOGNITION

Advertisements, 1st set	2.1 to 1
Words	2.1 to 1
Mutilated advertisements	2.1 to 1
Geometrical forms	1.9 to 1
Advertisements, 2nd set	1.8 to 1
Syllables	1.5 to 1
Forms, 2nd set	1.4 to 1
Simple pictures	1.1 to 1
Average	1.8 to 1

The ratio of about 2:1, which is found in these cases, is found in so many experiments on various sorts of mental capacity that it cannot be entirely without significance. Professor Cattell calls attention to numerous cases in which this ratio of best to poorest is present. It should not of course be taken to indicate any necessary distribution of capacity among

human beings in general, for the range here is much greater than that represented by this ratio. What it does seem to indicate is that, when individual observers are chosen from a social group which is formed on the basis of some more practical consideration,—as college students, engineers, 8th grade children, etc., a range larger than this will not usually be found. This seems to mean that a ratio of two to one, as between best and poorest, includes those individuals who can satisfactorily and practically find a permanent place in the given group. A more extreme variation, either above the best or below the poorest, is sufficient to place the individual in another social group, and he is not likely to be included among our observers unless special care is taken to find him.

Summary

1. A closer study of recognition than has been made heretofore will probably contribute much to our knowledge of other processes as well.

2. A schematic account of the mechanisms of recall and of recognition seems to involve a common neural pattern, operating in reverse "directions" in the two cases.

3. Purpose, intention, and similar determining tendencies, are much more effective in recall than in recognition.

4. The value of a single presentation is greater in recognition than in recall, and the difference between the values of repetitions becomes still greater the more meaning the material possesses.

5. Recognition is based on varying degrees of assurance, and the degree of this assurance is a fairly definite index of the accuracy of the recognition. Assurance varies with the individual, the material, the length of series, the integrity of the items, the time intervening since original presentation, etc.

6. Assurance and correctness are higher the farther the content is removed, in character, from nonsense material.

7. Primacy and recency influence recognition much as they do recall, and, under certain conditions at any rate, primacy is stronger than recency.

8. A definite "recognition span" may be found, which will vary with numerous individual, material and technical conditions.

9. When the attempted identification is not immediate but takes place at longer intervals after original presentation, various sorts of materials show characteristic differences in relative recognisability and in effect of increased interval.

10. The influence of increased series length varies with the material, and stands, in general, in inverse relation to the meaningful character of the material.

11. The range of individual differences in fidelity of recognition within a given socially selected group is similar to that found in the case of other individual differences.

12. The indications are that a range of about 2:1, as between best and poorest, includes the limits of variation which common experience imposes on the selection of a practical or social group.

13. Other interesting facts concerning recognition have been suggested, such as the failure of retroactive inhibition, relatively small influence of increase in series length, etc.

A NOTE* ON THE RELATION AND ÆSTHETIC VALUE OF THE PERCEPTIVE TYPES IN COLOR APPRECIATION

By E. J. G. Bradford

Abstract

The preface of this note consists of a brief statement of certain experimental results bearing on the order of preference for certain colors, together with the reliability of this preference order, and on the frequency of occurrence of the perceptive types among university students. An analysis of the four principal perceptive types suggests that there are two distinct types within the one which has been called "Associative," namely the 'Sensational-Associative' and the 'Emotional-Associative.' Physiological factors are advanced as likely to affect the perceptive type of an individual. The æsthetic value of the types is considered on the basis of the above mentioned analysis.

The substance of this note deals with certain general considerations concerning the 'perceptive' types and their relations. These considerations have arisen incidentally in the course of some experimental work; hence only the briefest summary of the methods and results of the experiments is here necessary as an introduction to the theoretical matter which follows it.

The Experiments

The Subjects.—These consisted of twenty-six university students, eighteen of them graduates; thirteen from each of the Arts and Science faculties.

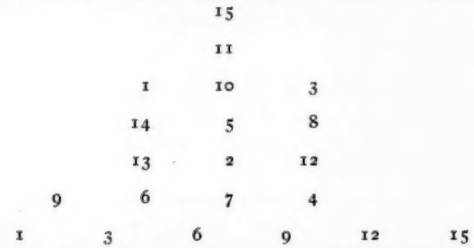
Method.—The subjects were presented with a set of fifteen rectangular pieces of paper each about 30 square inches in area. These papers were numbered in order to avoid difficulties arising from individual differences in color naming. The colors were arranged in a row and the subjects were

* From the Psychological Laboratory of King's College, London, England.

instructed to write down the numbers of the colors in order of preference. When this had been done the next instructions were given, namely to write down against each the reasons for liking or disliking that color.

The Results

Preference Order.—In order to ascertain the average preference order the following procedure was adopted. The fifteen possible positions of a color were divided into five groups whose limiting positions were 1-3, 4-6, 7-9, 10-12, and 13-15. The median position for each color was found, these medians were then grouped together according as they themselves fell within these same limits. The fact that no color has a median position lower than 10.2 confirms the general impression of the subjects, namely that on consideration hardly any colors are really disagreeable when taken by themselves.



In the above polygon the columns are proportional to the number of colors whose median positions fall between the limits mentioned. The colors were as follows: No. 9, dark blue; No. 1, saturated green; No. 14, chocolate brown; No. 13, pale blue; No. 6, slate grey with bluish tinge; No. 15, saturated crimson; No. 11, pale green; No. 10, coffee brown; No. 5, bluish green; No. 2, ink red; No. 7, cinnamon brown; No. 3, pale pinkish brown; No. 8, bluish green; No. 12, pink; No. 4, yellowish green. The figures inside each column of the polygon are the numbers of the colors falling in that group, in order of preference reading from the top downward.

An inspection of the polygon will show that loss of saturation tends to lower the position of a color *e. g.*, dark blue No. 9 comes within the first group while No. 13 comes within the second; Nos. 14 and 10, 7 and 3 are further instances. The admixture of a small proportion of another color has a similar lowering effect, *e. g.*, Nos. 5 and 1, also 14 and 7. Regarded objectively, the first eight colors in the

preference order are pure, while the last six seem to be adulterated with some foreign element.

Cohn had previously put forward the view that increase of saturation tended to make a color more pleasing,[1] although Major[2] obtained results which were antagonistic to those of Cohn. The above results tend to confirm Cohn's results although the details of the method used were different.

Reliability of the Preference Order.—Three subjects were again tested for their order of preference after considerable intervals, in order to determine the constancy of their preferences. A second estimate was obtained after an interval of fourteen days and a third estimate after a lapse of twelve months.

Subject	Correlation coefficients between		
	1st and 2nd	1st and 3rd	2nd and 3rd
H. S.	$.81 \pm .06$		
E. B.	$.94 \pm .02$	$.87 \pm .04$	$.88 \pm .04$
L. S.	$.94 \pm .02$	$.81 \pm .06$	$.83 \pm .05$

The formula used was $\rho = 1 - \frac{6 \Sigma d^2}{n(n^2 - 1)}$

Differences of Perceptive Type.—In order to see if training influenced the mode of color appreciation, the subjects were classified according to their faculty. The total number of answers given by the thirteen subjects in each group were classified and the results were as follows:

Group	Physiological answers	Objective answers	Associative answers	Character answers
Arts.....	59	34	35	35
Science.....	40	48	40	32

Although the differences under Physiological and Objective answers are big, they can hardly be regarded as significant since in neither case is the mean greater than twice its probable error.

[1] J. Cohn. Gefühlston und Sättigung der Farben, *Philos. Stud.*, XV., 1900, 279-286.

[2] D. R. Major. "On the Affective Tone of Single Sense Impression," *Amer. Jour. Psychol.*, VII., 1895, 57-77.

[The value of applying mathematical methods to experiments dealing with æsthetics is questioned by some workers in this field. However, if averages are employed, there should be some check on them before they are made the basis of any conclusions. Take an example from some fairly recent work. Winch[3] bases his conclusions regarding the color preferences of school children, on averages which at first sight appear large but which on investigation fail to reach the required standard of significance, in other words they are less than three times their probable error.]

The 'Perceptive' Types.—A psychosis may be regarded either from its cognitive or affective aspect, and this is most certainly true of those psychoses which give rise to an appreciative judgment or opinion. The perceptive types in æsthetic appreciation fall naturally into two groups according as the sensational or emotional elements play a predominant part in deciding the subject's attitude towards the color. Sensational elements are to the fore in cases where the subject gives an Objective or Physiological answer, whereas it is the emotional elements which predominate when Character and some Associative answers are given. This difference will be made clearer by considering each type singly.

In what may be called the Physiological psychosis there is a complex of sensational elements (visual, tactile, kinæsthetic and visceral sensations and images) together with their emotional accompaniments. These sensational elements are purely personal; they are identified with the subject's own bodily sensations and are characterised by a minimum of objectivity. The sensational character of the Physiological answer does not depend necessarily on the absence or weakness of the emotional elements but rather on the fact that the subject has not reached a sufficiently advanced stage to enable him to differentiate the emotional from the sensational elements in consciousness, and hence to give a bias to either one.

There seems to be no *a priori* reason to assume that the emotional elements are stronger in a psychosis which leads to a Physiological answer, than in one which leads to a Character answer; the change from one type to another depends rather on an increasing power of introspective analysis than on an increase in the strength of the emotional elements. Introspection in the former type makes evident the

[3] W. H. Winch, Color Preferences of School Children, *Brit. Jour. Psychol.*, .III, 1909, 42-65.

sensational elements which are far more easily noticed than are the emotional complexes which form the basis for the latter type of judgment. Differences in the strength of these emotional elements are due to temperament, and temperament, it is generally admitted, is not in the normal subject liable to change during a lifetime; yet subjects who give two types of answer to the same color and who seem satisfied only when two answers have been given can hardly be expected to have changed temperamentally during so short a space of time. For example a particular shade of yellow is called sickly when it is obvious to an observer that the subject who gives this answer, is strongly affected by the emotion of disgust. Another subject when presented with the same color might say that for him this color typified the emotion of disgust, a sort of hypostatised "disgust," a Character answer. Why this difference of answer when both subjects experience the same emotion? It may be argued that the word 'sickly' is more often used by the first subject than is the word 'disgust' and hence a difference in type is really only a sign of a difference between the affective vocabularies of the two subjects. The fact that the word sickly is more commonly used by the one subject than by the other may be taken as good evidence that this subject is limited by his inability to differentiate the sensational from the emotional elements, and that it is this lack of ability in differentiation and not his vocabulary that is the prime cause of a difference of type. Character subjects are often in difficulty when asked to express this appreciation in words, those who can do so are registered as of the Character type, those who cannot, 'break down' to a lower type because of their inability to find a word quite suitable to their state of mind; not being able to fix an emotional complex with a word, they fall back on the more easily defined sensational elements.

The above mentioned differentiation may be accomplished in either of two ways, which result respectively in two types, the Objective and the Associative. The Objective subject separates these elements by totally neglecting one of them. The sensational elements (color sensations) seem to be attracted from the self to the object and, as it were, replace that object and thus are regarded as being quite impersonal; the object thus becomes of no intrinsic value so far as the judgment is concerned. With the Associative on the other hand, the association of mental elements with the object tends rather to draw the object towards the self, and no 'depersona-

tion '[4] of the elements results. The object as such becomes an integral part of the complex which determines the appreciation, and in some cases the introduction of the object seems essential to the creation or augmentation of the emotional tone.

Answers which are classified as Associative, really differ very much among themselves when examined introspectively. There appear to be two types grouped together under the head "Associative." The distinction between them depends on the predominance of the sensational or the emotional elements respectively. For example, a red color will suggest a 'red flag,' because of their common sensory element, by a process somewhat akin to complication, *e. g.*, a red flag is more than a red patch; it has substance, and hence it implies touch sensations or at least a combination of perspective and color sensations. On the other hand it may be that the red excites the same emotional complex as does the 'red flag,' in this case the emotional elements predominate and although it may not be strictly correct to say that they act as the medium of association, yet they do seem to favor that particular association. (There seems to be some justification for this last assumption when abnormal subjects are considered, for it is well known that the memory of an alternating personality is dependent on the emotional tone of the subject at that particular time.) These two forms of the Associative type may be called the Sensational-Associative and the Emotional-Associative; the former might possibly develop into the latter because the introduction of the object might tend to reinforce the emotional elements in consciousness, and thus reinforced the subject would more easily distinguish them.

That some colors are more potent than others in arousing the emotions is undoubted, and it is these colors which first enable the subject to differentiate out the emotional elements which once noticed, as it were, almost in vacuo, will be more apparent when encountered in less easily analysed complexes. This provides one way in which advance from a lower to a higher type can take place. However, the same color is not equally potent for all subjects, probably because of the differences in sensitivity of the color apparatus of the eye.[5] For

[4] Bullough has suggested the term 'exteriorisation' as a label for this process. It is admittedly a rather barbarous term but it is difficult to find another which is equally suitable unless "depersonation," which term is here used.

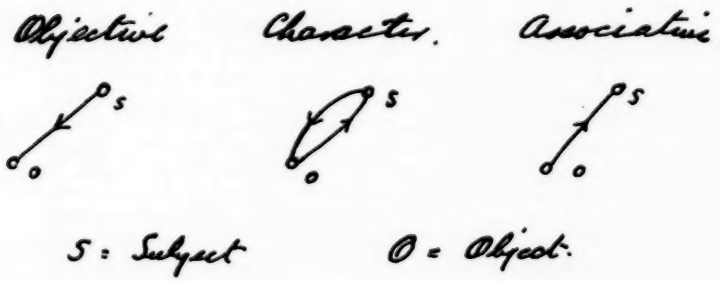
[5] Since the above was written it has been shown by experiment that fairly large differences of sensitivity to color exist. (See L. R. Geissler, Experiments in Color Saturation, *Amer. Jour. Psychol.*, XXIV., 1913.)

instance a red which is styled "glaring" by one subject, may by another be called "cosy," because the red apparatus of the one is much more sensitive than that of the other. This difference in sensitivity may even be so great as to change the type of answer, thus a yellowish green might be "washy" for one subject, and "sickly" for another, because the one was more sensitive to the green than the other. The one would see a weak *green,* the other would see an impure *yellow* and not a green. It may be noted in passing that this same factor is probably largely responsible for individual differences in color naming. Past experience is another factor which influences the cognition of a color, especially those who are of the Associative type would tend to be influenced in this way. The effects of this factor are seen in such associations as 'green—vegetation' and 'yellow green—quinine' which also afford a good example of how closely connected the Perceptive types actually may be. The association 'green—vegetation' probably would be given by a subject in whose consciousness the sensational elements predominated; introspection alone could decide the validity of this assumption; whereas the association 'yellow-green—quinine' undoubtedly would be accompanied by emotional elements of far greater strength than those accompanying the former association. This second association shows the transition stage between the Associative type and the Physiological, from which by a process of association the mental content is pushed into the objective zone. The quinine association has a minimum of objectivity, it is still somewhat personal, a characteristic of the Physiological type, while the vegetation association is marked by far greater objectivity, in fact the personal element seems almost absent.

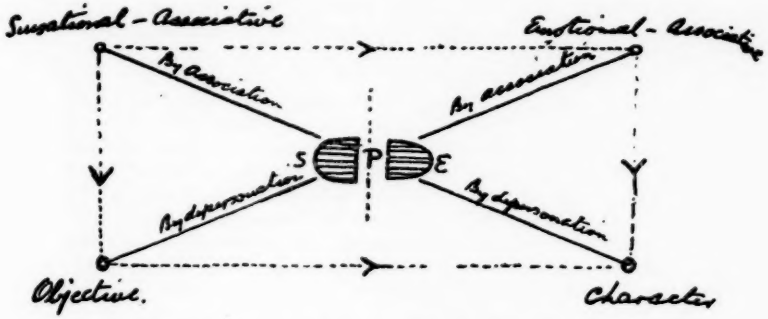
The Character type brings us to a stage where the emotional elements have successfully undergone the process of 'depersonation' either directly from the Physiological or indirectly by means of association. The Character type is like the Objective in that the process of depersonation releases certain mental elements from the self, but it differs in that it is the emotional and not the sensational elements which are thus liberated. For example, a pinkish buff, such as No. 3, by a Physiological subject would be called 'sickly,' his attention being largely to his own person rather than to the color. But an Objective subject would style it 'indefinite or impure, satisfactory neither as a pink nor as a buff,' thus showing how the sensational elements, color sensations, are considered as being intrinsically

a part of the color. The Character subject would say that it suggested 'baseness, meanness or fawning,' thus showing that this subject sees in the object a certain emotional tone which is really part and parcel of his own consciousness. Besides the similarity due to depersonation of the mental elements the Character and Objective types both have the characteristic of evaluation though their standards be quite different, the former has an ethical and the latter an hedonic standard.

Physiological subjects react to a color with motor manifestations, *e. g.*, facial expressions, and these seem to be the sole effect of that confused complex of sensational and emotional elements of which the Physiological psychosis is composed. The conative tendency of an Associative subject seems to be entirely satisfied by the uncritical acceptance of some object appropriate to the state of mind induced by the color presented. The conative tendency in the Objective subject is not satisfied by the analysis of the sensational elements; he must proceed to the simple judgment of good, bad or indifferent. Character subjects take up an attitude of evaluation towards the emotional elements ascribed to the color, and by a process of hypostatisation are led to react towards the color as if it were a person possessed of these same qualities. For instance the subject when in the presence of a person who is 'hard and keen' has a particular emotional disposition in a nascent condition; a color which is 'hard and keen' causes him to feel sympathetically 'hard and keen' besides feeling somewhat affected by the previously mentioned disposition. Thus there is first a movement towards objectivity followed by a movement towards subjectivity comparable to the two processes manifest singly in the Objective and Associative types respectively.



To summarize, it may be said that the four types of appreciation all develop from the Physiological by the application of two different processes to the two species of mental elements found in consciousness.



The above diagram shows graphically the relations between the various types.

S = Sensory elements. E = Emotional elements.
P = Physiological.

The dotted lines are to show that the Character type may be approached directly from the Physiological or indirectly through the Objective or Associative channels. The justification for this assumption is that certain Character subjects gave two reasons for appreciation, sometimes the second reason was Physiological, sometimes Objective and sometimes Associative; showing that in some cases at least the character attitude was nearer to the Associative and Objective than it was to the Physiological.

The Æsthetic value of the types. It is rather difficult to classify the types according to æsthetic value because both the attitude of the subject and the content of his consciousness are involved.

It seems safe to say that the Sensational-Associative type has no value at all, since it is characterised neither by its emotional content, by the differentiation of mental elements, by abstraction, by depersonation nor yet by the fusion of the differentiated elements with the self.

The Physiological type is characterised by a psychosis in which the emotional elements are present but remain undifferentiated.

The Objective type, so far as mere attitude goes, is closer

to that of æsthetic appreciation than are the previous types, in that it is characterised by an initial depersonation but the process has been applied to the wrong material.

The Emotional-Associative has a claim on the ground of the content of its consciousness; yet the interposition of an object as a means of releasing this content from the self seems hardly to be the correct method to lead to the attitude of æsthetic appreciation.

The Character type alone has any claim to the title 'æsthetic;' and even it lacks that relation of fusion between the self and the object of appreciation, which partly at least depends on the emotional sympathy between the object and the self. It is certain that the Character type is the most highly developed of the types. The Objective and Emotional-Associative are partially developed and hence can fairly be said to rank next. The Physiological represents the crude undeveloped content of experience, while the Sensational-Associative can hardly lay claim to be called 'appreciation' at all.

The order which suggests itself from these considerations is the following:

1. Character.
2. { Objective.
   { Emotional-Associative.
3. Physiological.
4. Sensational-Associative.

Reading downwards from those of most to those of least æsthetic value.

THE COMPARATIVE VALUE OF VARIOUS CONCEPTIONS OF NERVOUS FUNCTION BASED ON MECHANICAL ANALOGIES*

By MAX MEYER, University of Missouri

The word "mechanical" in the title of this paper is used in its widest sense, as meaning anything machine-like. The advantage of mechanical analogies in our conceptions of the functioning of the nervous system consists in their forcing us to think in quantitative terms. We psychologists, and also the physiologists and zoologists who are interested in our problems, have been accustomed too long to think exclusively in qualitative terms. Imagine an engineer designing (and true understanding is equivalent to designing) a machine by thinking in such purely qualitative terms as this: the rise of temperature in the boiler is "instinctively" or "reflexly" followed by a rotation of the fly-wheel; each movement of the crank through one hundred and eighty degrees "associates itself" with one movement of the valve gear; the strokes of the piston-rod are "inhibited" by a raise of the damper lid on the flue, etc. The engineer can design a machine only by thinking of a fly-wheel of a definite shape and weight, of a piston-rod of a definite length, of a piston of a definite cross-section and length, of a boiler of a definite size, of a definite degree of temperature within it, of a definite quantity of coal, etc. We shall never understand the function of the nervous system unless we are willing to think of it, too, in quantitative terms. We can use quantitative terms equally well in either of two ways, by means of geometry, and by means of arithmetic and algebra. A quantity or a quantitative relation can be defined equally well in a graph and in an equation. A description of a nervous function which employs neither a graph nor an equation, but humors the reader who dislikes mathematics by confining itself to qualitative terms, is not worth the paper on which it is printed.

This problem has stood in the center of my interest for more than ten years, although I have expressed myself on it

* Paper read at the meeting of the Western Branch of the American Psychological Association, Easter 1913, at Evanston, Ill.

only during the last five. But others have been at work on this problem, too. You have undoubtedly heard of Uexküll. He does not, however, offer much of interest to the psychologist because he has devoted himself chiefly to the explanation of the behavior of such animals as insects and still lower forms, whose nervous systems are undeveloped in comparison with that of man. His mechanical analogies are, in my opinion, neither applicable to higher functions, as, for example, to learning,—the central fact of psychology,—nor are they simple enough for the demands of science, nor are they always easily translatable into anatomical and physiological terms. I agree, on the whole, with Jennings who expressed the following opinion: "It seems that, even for practical purposes, the author (Uexküll) has overestimated the value of a rather gross 'Anschaulichkeit.' The bringing in of machine-like structures,—tubes, valves, etc.,—that confessedly do not exist, seems rather to confuse than to aid the mind. It is not possible to conclude directly from the properties of the assumed machines as to what physiological properties one will find, for the parallelism is far from complete, so one must try to keep the system of machinery separate from the system of physiological facts; there are two systems to grasp instead of one."[1] Let me say at once that I did not derive any of my ideas on this subject from Uexküll.

Mr. Nathan A. Harvey, of Ypsilanti, Mich., has been working along similar lines. But the chief problem of psychology, that of learning, became a stumbling-block to him. He announces his renunciation in the following words: "There is one thing that is not perfectly plain in this scheme: No explanation can be thought of, at least by the present writer, that would account for the shifting of the dendrites in the process of attention. Why should the dendrites move? Here is a place where no answer can be given, and when a person points out that this involves some supernatural agency and cannot be accounted for in terms of pure physiology, we have to acknowledge that we see no way of accounting for it."[2]

Quite recently Mr. S. Bent Russell, an engineer of St. Louis, has offered some interesting ideas on this subject. The

[1] H. S. Jennings, The Work of J. von Uexküll on the Physiology of Movements and Behavior. *Jour. Comp. Neurol. and Psychol.*, XXIX, 1909, 330f.

[2] Nathan A. Harvey. A Device by which Physiological Concepts may be employed in teaching Psychological Functions, *Western Journal of Education*, IV., 1909, 182.

last part of this paper shall be devoted to a comparison of the functional concepts as published by Mr. Russell[3] and those published by myself in my book on the Fundamental Laws of Human Behavior (1911). In some respects I agree with Mr. Russell; in others I disagree. Whatever our differences may be, however, I wish to express the opinion that Mr. Russell has done science an invaluable service by the publication of the paper in question. He describes in all structural details a hydraulic machine which, if inserted in a steamship, would reveal among other possibilities the one described in the following tale.[4]

A canal, large and deep enough to carry a steamboat, has the shape of the numeral 8, or, more strictly speaking, of two circles touching each other. At the point of contact, where the waters belong to the one circle as well as to the other, a steamboat is floating, in the direction of the tangent of both circles. On the deck of the steamboat there are three levers, No. 1, No. 2, and No. 3. No. 1 starts the propeller driving the boat straight ahead. No. 2 starts a second propeller which, serving instead of the rudder, gives the boat a turning movement to the left. No. 3 starts a third propeller which gives the boat a turning movement to the right. On deck, before the three levers, stands one of those psychologists whom I criticized in a paper read before this association a year ago,[5] who believe that a machine can change its ways of working only if a ghost is kind enough to step in, disconnect something, and connect it with something else, —in other words, who believe that "experience" without a ghost is unthinkable. It is a fine Sunday morning, and our experimental psychologist begins his pleasure drive by pulling lever No. 1. But soon he observes that the boat is straightway approaching the dividing point of the canals which threatens destruction. He pulls lever No. 2; the boat swings to the left and follows the circular route of the canal without touching the shore. After a while the opposite point of the circle is reached. It is lunch time. Our psychologist pushes both levers, No. 1 and No. 2, back, and the boat stops. After lunch, to continue his trip, he again pulls lever

[3] S. Bent Russell, A Practical Device to Simulate the Working of Nervous Discharges. *Journ. Animal Behavior,* III, 1913, 15-35.

[4] The tale is not given in Mr. Russell's paper, but, I hope, will appeal more strongly to the imagination than Mr. Russell's equivalent, but somewhat dry, description in engineering terms.

[5] Max Meyer, The Present Status of the Problem of the Relation between Mind and Body. *Journ. Philos. Psychol. and Sci. Meth.,* IX, 1912, 365-371.

No. 1. Suddenly he remembers that No. 1 serves the forward propeller, and looks ahead to see if he is dangerously near the shore. To his amazement he finds that the boat is following the circle properly. Not only is the forward propeller working; the propeller turning to the left is working too, although no one has pulled lever No. 2. The steamboat has an engine which "remembers." After some time the point of departure is reached. Lever No. 1 is pushed back, the boat stops in its original position, and our experimentalist goes home.

The following Sunday we find him again on deck. He pushes lever No. 1 and the boat starts. But the engine no longer "remembers." After so long an interval as a week it "forgets." The boat moves straight ahead. Our adventurer pulls lever No. 3; the boat swings to the right and follows the route of the other canal. Lunch time comes again. Both levers, No. 1 and No. 3, are pushed back, and the boat stops. After lunch the experimenter pulls lever No. 1. The engine "remembers." The boat does not go straight ahead, but follows the curving of the canal to the right. The forward propeller and also the propeller turning to the right both work, although lever No. 3 was not touched again. But after a week the engine "forgets" this experience, too. Nothing, however, prevents it from relearning.

Now, this is not altogether a fairy tale. This wonderful engine (which, after all, is no wonder) is described in sufficient detail in Mr. Russell's paper and, in summary form, it is spoken of on page 30 in the following words: "Figure 6 and table III. show a duplex converging gang. In this arrangement the first key rod (or sensory terminal) *SS* is known as the station key. *MR* and *ML* are opposite movements. If *SS* and *SL* are habitually struck in succession, except when *SS, SR* and *SL* are struck in succession, the device will become "trained" so that when *SS* is struck the movement *ML* will result. On the other hand if *SS* and *SR* are habitually struck in succession the key *SS* will when struck give the opposite movement *MR*." Nothing, except the limitations of your pocketbook, prevents you from having such a pleasure yacht built for your own use in vacation hours. You simply have to follow the specifications given by Mr. Russell in that part of his paper which is devoted to their exposition, beginning on page 21 with the heading "Operation" and ending on page 30 with the words just quoted.

In the pages from 31 to 35,—the closing section of his

paper,—Mr. Russell gives specifications for hydraulic machines capable of still other varieties of learning by experience, in fact, of most of those varieties which I have enumerated in my book on Human Behavior. And nowhere in his specifications is any ghost to be found. The specifications contain objective realities exclusively.

What, now, is the value to pure science of these pages, from 21 to 35, of Mr. Russell's paper? Of course, their value to the millionaire who desires to own a pleasure yacht which remembers and forgets its experiences, is obvious, but merely practical. Their value to pure science, as I see it, consists in the fact that we have here a demonstration of the possibility of an "organism," capable of learning and forgetting, which obeys no ghost whatsoever, but only the laws of mechanics. This possibility has been denied by those psychologists, led at present by William McDougall, who have written "Interactionism" on their standard. They might perhaps, in order to save their position, reply that S. Bent Russell has proved this possibility only for a mechanical, but not for a biological organism. But is this not a purely technical evasion of the point at issue? We have always accepted almost like a dogma,—and shall continue to do so,—that what is possible in "mechanics" (taking this word in a narrow sense as the science of matter and motion, but including hydromechanics) is possible in physics-chemistry, the larger field of science; but not the reverse. And we further believe that what is possible in physics-chemistry, is possible in biology; although the reverse may not be true. If it is proved that a mechanical organism can learn and forget without the interaction of a ghost, we have no right to assert that a biological organism can not.

Is Mr. Russell's demonstration of a learning and forgetting machine also valuable to science in suggesting new experimental work in physiology, anatomy, zoology? Here, unfortunately, its value appears very limited. The objections raised by Jennings against much of the theoretical work of Uexküll, apply with equal strength to this of Russell. What suggestions does the neurologist receive from a multitude of terms like these: spur valve, pawl, pawl spring, ratchet valve, rocking finger, finger lever, plunger, dash pot barrel, balanced slide valve, key rod, bell crank, suspender link, coupling gang, etc.? I am far from asserting that none of these terms is translatable into neurological terms; but I do not doubt that most of them are untranslatable.

Let us now turn to a consideration of the first pages, not

referred to as yet, from 15 to 20, of Mr. Russell's paper. I propose to compare very briefly our quantitative concepts of nervous function. As previously explained, the geometrical drawings are an essential part of this concept. I represent the nervous system as consisting throughout of arches, parallel or superposed, each arch consisting of a rising, a connecting, and a falling line. I have also devised a consistent method of lettering; but this concerns us less, at present. Each line represents a neuron. This does not mean that in the anatomy of an animal it may not denote several neurons. But no fewer than three can compose an arch. It appears immediately to the eye that some arches are reflex arches, others, the higher arches, are "nerve centers." Regarding the resistance of each neuron as originally the same, it appears immediately to the eye that certain points (sensory and motor) are "corresponding," that is, connected by a nervous path of the smallest possible (length, and therefore) resistance, and that other points are connected only by way of superposed arches, by paths of greater resistance,—how much greater is again immediately clear to the eye.

Mr. Russell uses a different kind of diagram. Where I use an arch of three lines as representing a reflex arc, he uses an angle of two lines. Where I use an arch of three lines as representing a superposed nervous arc, he uses a diagram of four lines. He seems to prefer this method of drawing the diagram on account of its showing all the sensory points on one (the upper) side of the diagram, and all the motor points on another (the right) side. I am not certain that this is an advantage. But this method, in my opinion, has the decided disadvantage that one cannot build up systematically an unlimited number of higher and higher nerve centers without making the figure confusing, for all the higher centers of different level would be mixed up among themselves and with the reflex arcs. In my method all the arcs representing the same nervous level, that is, the same closeness or remoteness of connection of a sensory with a motor point, appear clearly to the eye as standing on the same level in the diagram. Having counted the number of neurons from one of these arcs to the sensory or motor periphery, we know it for all other arcs on the same level. Counting, indeed, is not necessary even in the first place, since my method of lettering gives the answer without counting. It is not strange, then, that Mr. Russell, in his stream diagrams, restricts himself to one level above the reflex arcs. That the application to animal behavior demands the removal of such

a limit of the number of levels, I have shown in my book, for example, on page 44.

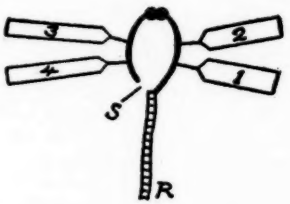
Mr. Russell seems to believe (p. 19, and also the last paragraph of his paper) that he has given an explanation of the mechanism of the association of ideas. I do not concede this. In order to give such an explanation, it is necessary to make first a definite assumption with respect to the nervous correlate of a mental state. I have made such an assumption (on pages 233 ff.) and I have given the explanation: Mr. Russell has not.

A similar remark may be made with respect to what Mr. Russell says on page 33 about satisfaction: "This mechanism . . . illustrates . . . satisfaction, showing how associations may be reinforced or stamped in." I have stated the nervous correlate of feeling on pp. 238-239 of my book.

Mr. Russell, throughout his paper, uses the term "susceptibility" in a sense entirely different from that in which I have consistently used it. I mean by (positive and negative) susceptibility (p. 115) of a neuron its ability to respond to a flux by an increase of conductivity, to lack of function by a gradual return to the former conductivity. Mr. Russell, however, does not use the word susceptibility in this sense of "being capable of a gain or loss in conductivity," but in the sense of a synonym for conductivity itself. He uses, in addition, capacity as a synonym for the same term, conductivity. It is my opinion that no synonyms are needed for conductivity, and that the use of either susceptibility or capacity in this sense is confusing.

To make plain the different ways in which the nervous processes streaming through the diagram influence each other, Mr. Russell uses the analogy of a hydraulic machine consisting of fifty or a hundred parts, of some of which I have quoted the names. These parts of the machine, essential for its functioning, mean practically nothing to the student of neurology, suggest no new experiment. Let us compare the mechanical analogies which I have used. I assume that the point of contact between the ends of two conductors serves (normally) as a one-way valve (called check valve by the engineer). The translation into neurological terms is simple. We assume that the so-called synapse, much abused in psychological theory, when normally functioning, permits the passage of the nervous excitation in one direction only. I further make an assumption about the relative susceptibility in lower and higher arches, which hardly needs a translation into, but is practically stated in, neurological terms. This assumption

directly refers to certain known facts of neurology and animal behavior[6] and suggests further experimental work in these sciences. I also introduce (and this is practically all the mechanical analogism which I use) the two concepts of deflection and overflow. The latter means this. Think of a metal wire of a given electrical resistance. If its temperature rises, its resistance becomes greater; and the rise of temperature may be due to the current itself. Suppose something similar holds true for a neuron carrying a nervous excitation,—there you have the translation, simple and direct, into neurological terms. The consequence would be of the nature of the function which I have called overflow, that is, certain rather long paths would carry a greater flux than was to be expected from the original computation of the resistances. And this plays a great rôle in the explanation of what I çall instinct.



Deflection of a stream by another, stronger stream, drawing the weaker one into itself, is easily demonstrated by means of an apparatus which anyone can build out of two glass T's, a glass jet, a few feet of rubber tubing and a bottle of ink, the whole to be connected with the water faucet. I do demonstrate this, for the sake of "Anschaulichkeit," to use Uexküll's phrase, every semester to my students. The translation into neurological terms is self-evident, if we are willing to think of the nervous process as a streaming of ions and of the nervous architecture as corresponding to the general features of my diagrams. Let no one think that the experimental proof of such a nervous "deflection" is a doubtful possibility of a remote future. The deflection was demonstrated ten years ago by a brilliant experiment of Uexküll.[7] In our figure is seen the anterior part of the nervous system of a worm. It is artificially severed at *S*. 1,

[6] *Cf. Human Behavior*, 113-115.
[7] J. von Uexküll, *Ergebnisse der Physiologie*, III, 2, 1904, 10.

2, 3, and 4 are muscles which have a connection of low resistance with the ventral cord, but a high resistance connection with the head ganglion. If the head ganglion is stimulated, the muscles 1, 2, 3, and 4 respond, but weakly, owing to the high resistance of the nervous paths. If at the same time the ventral cord is stimulated at R, 1 and 2 respond strongly, of course, but 3 and 4 respond still more weakly. And if the stimulation of the ventral cord is made very strong, 3 and 4 no longer respond to simultaneous stimulation of the head ganglion at all. That is, the whole nervous process coming from the head ganglion, instead of partly going in the direction of 3 and 4, is now forced to join entirely the stronger nervous process coming from *R* and having its motor outlet in 1 and 2.

If you prefer a demonstration taken from the study of behavior, to an experiment made on a nerve preparation, think of all those facts talked of in the psychological text-books under the head of "attention." Why is it that the enthusiastic chess-player, walking home and at the same time occupied with his next move, suddenly stops in the middle of the street? Because the complex nervous process of preparing for his next move is so much stronger that it deflects the weaker nervous process which controls the muscles of the legs and thus keeps it from sending to these muscles the proper innervations.

I believe, then, to be justified in saying: The mechanical analogies which I have used are few. They are simple. Their significance is comprehensive. And they are easily translatable into physiological terms.

AN INTROSPECTIVE ANALYSIS OF THE ASSOCIATION-REACTION CONSCIOUSNESS

By EMILY T. BURR and L. R. GEISSLER

The present study was begun in the Psychological Laboratory of Cornell University during the Summer Session of 1910 and later continued by one of the writers at Columbia University. In the mean time the other published a preliminary report of some of the results obtained at Cornell,[1] and the present paper is to complete the account of the work.

In the earlier experiments an auditory presentation of the stimulus word was used; but this was later replaced by visual exposure, and the Hipp chronoscope took the place of the stop-watch. Otherwise the experiments were carried on in the familiar way. At the beginning of a series the observer had to choose between two pictures, or two stories, or two boxes containing a collection of articles, to familiarise himself carefully with the one selected, and then to seat himself before the apparatus. The material consisted of four pairs of short stories, four pairs of pictures, and two pairs of boxes. The members of each pair were as different as possible. For instance, a picture of a stone-quarry was paired with a picture of family life, and a story of hardship and tragedy was paired with a humorous, inconsequential tale. In the top of each of the boxes was placed an envelope containing written directions regarding the careful examination of the box. With each box was connected some joke or surprising incident. For example, the observer was told to unwrap a certain small package in one of the boxes and to examine carefully the lobster, which turned out to be an elongated convex mirror in which the observer saw his face reflected. Another box contained live flies in a small bottle, which was so arranged that the observer inadvertently let them out. Likewise the pictures and stories were selected on the basis of the strength of their emotional appeal, in order to make the complex as intense and vivid as possible. The observers were in each case comfortably seated, generally separated from the experimenter by a screen, and the room was so arranged that their attention was not distracted.

The first instruction given to the observer was: "Choose one or the other of these two materials (pictures, stories, or boxes), examine it carefully, and do not let the experimenter know which one you have selected." Immediately after the examination of the selected material the following instruction was given: "I am going to show you (or pronounce to you) one by one, a series of words, and you are to give as soon as possible, in response to each, a word that is associated in your mind with my word; but do not, if you can help it, give one that is connected with your choice. Then I want you to

[1] L. R. Geissler, A Preliminary Introspective Study of the Association-Reaction Consciousness, this JOURNAL, xxi, 1910, 597-602.

describe as clearly and concisely as you can your state of mind and its contents from the time of the ready-signal until you gave your associated word." To this was later added: "Give the mental events in their temporal sequence and try to give more details of the background of consciousness." The experimenter avoided all questioning and other suggestive remarks.

The observers at Cornell University were Miss E. Gunning (*Gu*), then Inspector of Schools at Amsterdam, Holland; and Messrs. G. C. Basset (*Ba*), R. Hugins (*H*), then Assistant in the Psychological Laboratory, Dr. T. Okabe (*O*), late Fellow in Psychology, and Dr. L. R. Geissler (*G*), then Instructor in the Department. At Columbia University the observers, with the exception of one senior in the Law School, were students in the Department of Psychology accustomed to serve as subjects in experimental work. They were Misses Ellison (*E*), Boshitsky (*Bo*), and Kupers (*K*), and Messrs. B. K. Fiske (*F*), and Myers (*M*). Altogether about one thousand association-reactions were obtained.

The quantitative results do not present anything new. Occasionally an irrelevant stimulus is followed by a delayed association, which is usually due to the fact that too many ideas rush through the background of consciousness and interfere with one another and with their articulation in a reaction. Such cases—they also occur where no instruction to conceal anything exists—are not so frequent as to influence to any great extent the average association-time for irrelevant stimuli. The longest reaction-times occur invariably with critical words; and their mean variation is considerably larger than that for irrelevant words. Critical stimuli were sometimes answered by quick and insignificant associations; but if followed up by one or two more critical stimuli, the complex could no longer conceal itself, and was manifested either in delayed or in significant associations or in both together.

Of much greater importance are our qualitative results as revealed in the introspections. They have led us to the conclusion that what is usually spoken of as the concealing of a complex is but a special case of a consciousness under negative instruction, the only essential difference being a greater degree of emotional vividness and strength in the complex. Consciousness under a negative instruction differs, as one of the writers has shown in a previous article,[2] from a consciousness under a positive instruction in that the former involves a stage of suppression and a conflict of at least two determining tendencies, while the latter requires only one such tendency. In order to prove our contention we may now compare the consciousness while trying to conceal a complex in the association-reactions, referred to hereafter as A.R. experiments, with the consciousness under a negative instruction, abbreviated into N.I. experiments.

The first similarity lies in the nature of the *Aufgabe* itself; for "not to betray oneself" is only one particular negative instruction. It does not even matter whether linguistically the instruction is formulated in a positive way; its intent is accepted negatively. When we remember how early and how frequently in childhood the instruction "don't" occurs, we need not be surprised at the readiness to respond to negative instructions. They are, furthermore, sanctioned by the principle of mental economy; for it is much simpler and easier to exclude one impossible way of action than to include all the possible

[2] L. R. Geissler, Analysis of Consciousness under Negative Instruction, this JOURNAL, xxiii, 1912, 183-213.

ways. Now it will be remembered that our particular instruction in the A.R. experiments accidentally emphasised somewhat the negative aspect by its linguistic form. The N.I. experiments, which were carried on a whole year later, were suggested by Langfeld's first article,[3] and their instructions were taken from his work, which *G* did not happen to see until his own preliminary report was published. It will be seen, therefore, that the two investigations were completed independently of each other, with no initial intention in *G*'s mind to compare the two consciousnesses. In fact, the similarity became evident only while the results of the second investigation were being prepared for publication.[4] We wish to emphasise this fact in order to exclude the possible suspicion that our first instruction was purposely framed in the negative form in order to produce an artificial similarity. But, even aside from this fact, it is self-evident that in any Freudian complex of concealment a negative task of suppression is involved, whether it be set by conventional rules of society or by the individual himself in accordance with his own nature, his own ideals and determinations. In all these cases the underlying thought is: I must not or I ought not to do so-and-so. Since, then, here as well as in our A.R. experiments the virtual instruction is 'not to betray oneself,' and in the N.I. investigation the actual instruction was 'not to name the picture or object exposed,' it should not be surprising that the consciousnesses involved in these cases are similar in all essentials, as the following descriptions and illustrations will show them to be.

Some of the introspections on the fore-period read, in the case of *O* for example, as follows: "My bodily posture was stiff and quiet; I held my breath, kept my right ear turned toward the experimenter, and had many organic sensations." At the signal 'Ready' he tended to assume an attitude of indifference, in which there was no particular attempt to control the mental processes (no one content of which was very vivid), and he found it impossible to localise the organic complex and the strain sensations. *Ba* says that "the effort to fix the attention" is accompanied by a strong tendency to assume some specific bodily attitude. Generally, therefore, at the signal 'Ready' he sits with his head resting on his right hand, his right elbow on his knee, his brows contracted, his jaws set, and his lips stiff. A very similar description is quoted in *G*'s preliminary report,[5] and the accounts of the other observers do not differ essentially from these examples. *H* experienced also many verbal ideas, as "I'm ready for the stimulus, let it come," or "I'm on my guard, I won't let her catch me this time," and the like. The women observers speak with greater emphasis of the "intense excitement" during the waiting period, and of "fears of self-betrayal" present in verbal form and "strongly unpleasant." In these descriptions we do not find a literal repetition of the instruction, but it is nevertheless represented either in a modified verbal form or in the whole bodily and mental attitude toward the experiment. There is one difference between these fore-periods and those of the N.I. experiments. In the latter, the fore-period became more and more mechanical, poor in conscious contents,

[3] H. S. Langfeld, Suppression with Negative Instruction, *Psych. Bull.*, vii, 1910, 200-208.

[4] L. R. Geissler, this JOURNAL, xxiii, 1912, 212 says: "A comparison of the consciousness under N.I. with the consciousness of the hidden complex is reserved for a future occasion."

[5] L. R. Geissler, this JOURNAL, xxi, 597.

and vague and faint in their degree. In the former, no such degeneration was noted, presumably because of the element of uncertainty, since not every stimulus was relevant, so that attention was kept alert. The same effect would probably have been produced in the N.I. experiments by the introduction of blank stimuli.

It had been found in the N.I. investigation that the mid-period could be subdivided into three stages, those of recognition, of suppression, and of suspense or search. The same thing is possible in the present work. In the first stage the stimulus-word is received and as a rule carefully examined as to its connection with the chosen material. For example, *K* says: "My first thought after any stimulus is given me is of the instruction. When I see that no connection can exist between the stimulus-word and the experimental material, I can find a reaction and give it with a deep sense of relief. When I realise that the stimulus is a critical one, I often have unpleasant blood pressure in my face, a palpitating feeling, and I say to myself, 'oh, that's dangerous.'" Similarly *Gu* says: "I try each word to see if it be a safe stimulus." *O* finds that he has mentally gone over the entire material, point by point, and when he realises that there is "no danger" in the stimulus, he assumes a relaxed attitude and the formation of a reaction is automatic. *H* says to himself in internal speech such phrases as "detective on my track, I mustn't incriminate myself" or "now we are on the path of the story, I must be careful." *E* seems to place herself each time in the situation suggested by the stimulus; she reads the story or event into every stimulus-word, and if it is not a critical one she usually has a verbal idea of "I'm all right, perfectly safe," and the tense strain and kinaesthetic sensations give way to "a sense of relief and peace." It is unnecessary to multiply the examples. The recognition of the irrelevancy of a given stimulus-word usually leads at once to an indifferent association.

As soon, however, as the connection between the stimulus and the chosen material is discovered, a stage of suppression sets in, or, as one observer says, "there is great difficulty in retaining a 'give away' reaction-word." This is to us the most important similarity between the two kinds of consciousnesses to be compared. *Gu* says: "The recognition that the stimulus is a critical one makes me strive to get away from the experimental material as quickly as possible," and likewise *E* writes that when a critical word appears she tries to get away from the influence of the dangerous material in order to find some non-incriminating connection. The other observers tell of similar suppressions of incriminating associations. *H*, for example, says in one case: "The knowledge of the picture seems to possess me like an obsession, and I can get no other thought." In other words, we have here the same two phenomena as in the N.I. experiments, on the one hand the failure of a negative instruction to keep forbidden ideas entirely out of consciousness, and on the other hand the successful avoidance of their motor discharge, that is, the inhibition of the reaction of articulation. Although some of the present introspections are not as detailed and technical as those in the N.I. experiments, it is nevertheless possible to divide most of these descriptions of the suppression into the two classes of attitudinal and ideational suppression that were found in the N.I. investigation. The attitudinal suppression, for example, is very strikingly intimated by *K* when he says that "the whole state of inhibition is something like the feeling one has when told by a doctor to say 'ah' while

he holds down the tongue with a spoon." Another illustration is given by *Ba* who describes the suppression as a "tendency to hold the breath, a rigidity of the muscles of the throat, accompanied by a fixed staring of the eyes," while four observers speak of their tongue as involved in the inhibitory process or mention tight closing of the eyelids, frowning and squinting. As examples of the ideational form of suppression we may quote *Ba*'s words: "Oh, I can't say this, for the stimulus is connected with the story;" and *K*'s introspection after the critical stimulus 'execution:' "In the background of consciousness there seemed to lurk, with uncompromising persistence, that danger reaction 'guillotine' and the verbal idea 'if you don't give that it will be hard to find another reaction.'"

The realisation of the connection between the stimulus-word and the selected material is described in most cases in terms of visual and verbal imagery. This realisation is, of course, one of the inessential differences between the two kinds of mid-periods compared, and is due to the peculiar experimental condition of distributing significant words among irrelevant stimuli. Another difference in the two stages of suppression is the stronger unpleasantness accompanying the effort to suppress an incriminating idea. *G* has described the feelings or emotions which in his case characterise the presence of a complex as "excitement and anxiety on the one hand, and relief on the other."[8] Other observers speak of "an indescribable state of anxiety," of "effort and strain," of "a feeling of helplessness," of "a feeling of breathlessness and a wild desire to escape from all ideas suggested by the critical word," of "a feeling of despair," and the like. The exaggeration of this emotional aspect in Freudian complexes, especially in subnormal minds, seems to have obscured a more critical and analytical view of the true nature of the consciousness of concealing, and to have prevented psychologists from seeing it in its normal aspects. It seems to us that the emotional feature is essential neither to the act of concealing or suppressing nor to the original complex itself which sets up the determination to conceal or to suppress. Whether the additional emotional aspect is responsible for most of the abnormal consequences of suppression is another question; but it seems to us that most of the complexes of normal everyday life are not marked by very strong feelings. The main emphasis must be put on the stage of suppression itself, and its conscious and (hypothetical) neural mechanism has been fully described in the previous article on consciousness under negative instruction.

It must be likewise understood that the stage of suspense or search, the last of the three stages of the mid-period, is only a by-product of the peculiar experimental conditions which require an association-reaction in the shortest possible time-interval. This requirement is a new instruction in itself, which sets up a separate determining tendency and which is interfered with in its normal course by the preceding stage of suppression. This stage of suspense is described by our observers in cases where no ideas will come into focus as "a groping attitude," "a floundering of ideas;" there is "a feeling of the awful passage of time," "a search for new connections." a situation in which "nascent processes seem all in a jumble, so that no one idea can be seized," a state in which "the light has been taken away and left a gray voidness," or "a period filled with vague, indefinite thoughts" for the expression of which the observer

[8] *Op. cit.*, 601.

is "unable to get any words, a stunned, helpless feeling." One typical concrete description of this whole stage of suspense may be quoted from *H*, who says "after the critical word 'sofa' was rejected, one verbal idea after another came. I felt each one, but could not form the corresponding words. Two or three seemed to come almost simultaneously, and it was hard to say which idea came first. A mood of indecision marked the entire period. I mentally compared the images, one with another, and the more I did this, the greater became my anxiety over the extremely long time. I was trying to form a reaction, yet I could not select any one of these images." Frequently the stimulus word is mechanically repeated several times in internal speech until a safe association arises.

The after-period in these experiments is again like that in the N.I. reactions, in that it contains the same kind of feeling of satisfaction and the same kind of self-justification or self-criticism with regard to the nature of the given response. *O* says, for example, "After having given a response, I try to analyse it again with reference to the significance of the stimulus word;" *E* says that she has "a feeling of justification after having spoken a word that could have no reference to the critical material;" and others make similar assertions. This shows that, as in the case of the N.I. experiments, "the mental processes of the after-period are still under the influence of the determining tendencies set up by the instruction."[7] How much, in fact, the whole consciousness is under their influence is easily seen from the external behavior of the observers during a critical experiment. The experimenter often noted contracted eye-brows, tightened lips, squinting of the eyes, raising and bowing of the head, clenching of the fist, restless movements of the feet, uneasy stirring of the body, deep breathing, and quick short laughs seemingly of relief that a successful response had been made.

We have now shown how close the parallel is between the consciousness of concealing a complex in the association-reactions and the consciousness under negative instruction, and we hope that this comparison has thrown some new light on the two phenomena. Whatever complications may enter into the consciousness of a Freudian complex, as met in everyday life or even in subnormal minds, they do not seem to us to be of such a nature as to obstruct seriously the subordination of the particular effort to conceal under the general task not to do a certain thing. In conclusion it may be pointed out that our results furnish fresh evidence for the fruitfulness and reliability of the recently much debated psychological method of introspection.

[7] *Ibid.*, 204.

"THE FEELING OF BEING STARED AT"—EXPERIMENTAL[1]

By J. E. Coover, Ph.D.

Summary

Experimentation with ten normal reagents to the extent of one hundred guesses each, as to whether they were being stared at during a fifteen-second interval, results in an astonishing approximation to the probability figure when the events are controlled by chance—namely, 50.2 per cent of right cases.

Introspective analysis reveals subjective factors that control the guessing and that confer upon guesses a strong feeling of certainty. These factors are sufficient to account for the commonness of the belief, and they are of the nature of incipient hallucinations and motor automatisms.

Introduction.—Titchener in an article in *Science*,[2] some years ago, stated that students in his junior classes believed they could feel anyone staring at their backs, that laboratory experiments proved the belief to be groundless, and that the belief is to be accounted for by *1.* a natural nervousness resulting from anxiety about how one's back looks; *2*, inhibition, due to the dictates of good-breeding, of the natural impulse to turn around to see if some one is staring; *3.* some one sitting behind, whose attention was attracted by the signs of nervousness, is caught "staring." It occurred to the writer, who had undertaken a general research upon the telepathic problem, that it would be of interest to know *1.* whether the belief above referred to is common with our students, also, *2.* whether it is shared by the men as well as by the women, *3.* what statistical results would yield in the case of a fairly large number of guesses by reagents who believe in the reliability of the feeling, *4.* and what factors in the experience in guessing would correlate with the greater degrees of certainty with which the guess is delivered. These factors, obtained from the introspections, would seem to be not only the essential processes at work when the reagent has 'the feeling of being stared at,' but also the elements of experience accountable, in part, for the belief that the feeling may be relied upon; they would then furnish an explanation supplementary to that given by Titchener in general psychological terms. In case the

[1] This is a part of the experimental work carried on in "Psychical Research" (endowed by Thomas Welton Stanford of Melbourne, Australia) in the Department of Psychology, Leland Stanford Jr. University, during the year 1912-1913. Acknowledgments are hereby made to Dr. F. Angell and Professor Lillien J. Martin of the department for courtesies extended to the researcher.

[2] E. B. Titchener, "The Feeling of being stared at." *Science*, viii., 1898, 895-897.

'feeling' is instinctive, as is suggested by the phylogenetic origin proposed by Titchener, these factors would serve to set off the feeling, to develop it in definiteness, to make it frequently active, and to give confidence in its reliability.

Replies to a questionnaire from 146 students out of a class of 169 in General Psychology, showed that 68 per cent have "the feeling of being stared at, with the conviction that the feeling can be (more or less) relied upon." Students of both sexes share the belief; 47 per cent of the writers were men and 60 per cent of them stated the belief.

That this class was not exceptional in possessing the belief is shown by 95 returns from another class of 102 in psychology (Mental Hygiene): 86 per cent of these students shared the belief; 54 per cent of the writers were men, of whom also 86 per cent stated belief.

Method.—Ten students, ranging from sophomores to seniors, from the class in General Psychology, who held the belief, made one hundred guesses each. The experiments usually occupied the reagent three or four laboratory hours, given on as many days, one week apart. The conditions usual in the psychological laboratory, of quiet, regularity of time and procedure, etc., obtained.

The interval of 15″ (20″ for the first five reagents) for forming the guess was begun by a pencil-tap by the experimenter and ended by two taps. After announcing aloud "yes" or "no" the reagent wrote out introspections involving *1.* condition of the mind during the interval; *2.* the kind, vividness, temporal course, and spatial attributes, of the imagery; and *3.* the grade and reason (if the grade was high) for the felt certainty of the guess. In order to avoid coincidence of similar mental tendencies in forming the series of experiments, the experimenter made up the series from a dice-box as guessing progressed. Upon recording the last guess he shook the box containing a die; if an odd number of spots were cast, he prepared to stare; if an even number, he prepared not to stare; and tapped off the interval in all other respects uniformly. Single experiments did not follow at a higher rate than one *per* minute; at the beginning of each reagent's work, until he became fairly critical in introspecting, they occupied from three to ten minutes each.

The reagent sat comfortably, with eyes closed and shaded with one hand, and with his back toward the experimenter. He knew that the series of experiments was controlled by the die-spots. When the experimenter stared, he 'stared hard' during the whole interval, 'willing' strongly that the reagent 'feel' it.

For reagents I-V the conductor of the research acted as experimenter; for reagents VI-X respectively there was for each a different experimenter, but of the same sex. Reagent I was the only man in the group.

Results.—The following table gives the results of both groups of reagents, and the averages.

TABLE I

Reagent	Not Staring		Staring		Total
	Total	Right	Total	Right	Right
I	51	28	49	22	50
II	45	25	55	26	51
III	62	23	38	20	43
IV	53	24	47	25	49
V	51	28	49	27	55
Avg.	*52.4*	*25.6*	*47.6*	*24.0*	*49.6*
VI	45	23	55	33	56
VII	56	21	44	23	44
VIII	48	25	52	26	51
IX	55	26	45	24	50
X	52	22	48	31	53
Avg.	*51.2*	*23.4*	*48.8*	*27.4*	*50.8*
Grand Avg.	*51.8*	*24.5*	*48.2*	*25.7*	*50.2*

Of the 1,000 guesses, 50.2 per cent were right (P.E., 1.78; M.V., 3.10); 47.3 per cent of the guesses when the experimenter was "Not Staring," and 53.3 per cent of the guesses when he was "Staring," were right. The die-spots came even 51.8 per cent of the 1,000 throws, conditioning this *per cent* of "blank" experiments.

Since six of the reagents guessed "yes" in excess of "no" (III. 18 times, IV. 8, VI. 10, VII. 16, IX. 6, and X. 22), while but three guessed "no" in excess of "yes" (I. 10, II. 8, VIII. 2), resulting in a general average of 6 "yes" guesses per 100 in excess of the "no" guesses, the excess of 53.3 per cent right guesses "when the experimenter stared" over the 47.3 per cent of right guesses "when the experimenter did not stare" is without significance; if half of the excess of "yes" guesses is deducted from the "Staring" experiments, the 53.3 per cent is reduced to 50.2 per cent. The total right guesses for each reagent is the significant figure. The limits are 43-56 and deviate from probability about equally; this size of deviation could be expected 322 times in 1,000.

Considering that theoretical probability is 50 per cent; that our result of 50.2 per cent ± 1.78 falls between it and the experimental probability found by Quetelet[8] in 5,460 drawings from equal numbers of white and black balls (white balls 50.48 per cent); and that an experimental series of our own (frequency of odd numbers on the dice) for the same number of experiments gives 51.8 per cent; we may conclude that no cause besides chance has been found working toward right cases.

There are other ways in which the results may be distributed to show that there is no conspicuous "bunching" of right cases in any of the rubrics, and that therefore the consistency of mutual support adds to the certainty that there has been no influence beyond chance operative toward right guesses.

In some of the experiments, the distance between the experimenter and reagent was varied for the purpose of finding the influence of

[8] Quetelet, *Lettres sur la théorie des probabilités*, p. 57.

distance upon any factor above chance that might be found to be working for right guesses. The following table gives the gross averages and the per cents of right guesses for the various distances in meters.

TABLE II

First Group

Distance	1	2	3	4.6	6	10 meters
No. Guesses	80	140	80	60	50	80
Per Cent Right	46.3	49.3	55	45	54	51.3

Second Group

Distance	2	4	6 meters
No. Guesses	160	100	20
Per Cent Right	45	53	50

But, as Venn says, anything may happen in a chance series, and it may be charged that all the guesses given with a low degree of certainty (a feeling that the guess stands a small chance of being right), by a freak of chance, may have run greatly under the probability-figure for right guesses, and may thus have counteracted in our final per cent for each reagent the influence of a force working for right guesses to be found in those guesses given with a stronger feeling of certainty.

A tabulation of right and wrong guesses under their correlated grade of certainty (recorded in the introspections), however, shows no significant advantage on the part of any reagent for his more certain over his less certain guesses. The following table shows a total of such values from reagents whose grading was definite.

TABLE III

Guesses Given with Various Grades of Certainty

Grades	A	B	C	D	"Pure Guess"	Total
No. Guesses	15	332	264	61	36	708
Right	10	166	129	33	22	360
Per Cent Right	67	50	48.8	54	61	50.8

It seems pretty clear that, if there is a capacity to be aware of being stared at, it is not, as Richet thought of telepathic phenomena, shared to a slight extent by normal persons, but must be confined, as James suspected,[4] to subjects whose sensibilities have been augmented beyond a "critical point" through hypnosis or other abnormal conditions.

Our reagents who had more or less confidence in their ability did not under the favoring conditionss of our experimentation prove their power. Their belief must be largely based upon those subjective factors which enabled them to deliver some guesses with a strong feeling of certainty, and partly perhaps upon undue consideration of cases in which they have "verified" their feeling by catching the starer.

[4] *Proceedings Society for Psychical Research*, 1896-7, 12:4.

Qualitative Results.—Introspections show in what manner guesses are determined, and reveal the factors of experience that contribute to the guesses a feeling of varying grades of certainty that the guess is right.

Certainty is contributed to the guess by (1) some attribute or content of the imagery, (2) kinaesthetic sensations or images, or (3) inferences from sound sensations resulting from the experimenter's manipulation of apparatus, etc., or from other subjective processes.

(1) When the content of visual imagery involved the attitude of the experimenter, it determined the guess according to whether the experimenter was looking straight ahead or looking away. When this imagery was vivid, or if it appeared with facility (liveliness) and at the beginning of the period, or was persistent or recurrent, the guess was given with a feeling of greater certainty. (Reagent I said, "when the direction of the look is seen from the face only, I give the guess Grade C; if from the eyes, Grade B or A.")

The visual imagery may be weak, and when it appears at all be accepted as a sign of being stared at. For Reagent II, visual imagery of the experimenter or of a school-room, in which she first experienced vividly the feeling of being stared at, yielded a "yes" guess.

Sometimes the content of the visual imagery was probably suggested by auditory impressions of the experimenter's movements when the latter were not pronounced enough to be singled out for "inferences" as treated in Section (3).

Those who depended largely upon visual imagery were Reagents I, IV, V, VIII, IX, and X.

(2) Some reagents were much occupied with kinaesthetic impressions during the interval. Thus for reagents III, VII, and VIII, the most characteristic cue for a highly graded guess was an almost irresistible impulse to turn around, or a tension of muscles in the neck and shoulders; for X it was the kinaesthetic sensation in the right temple; sometimes the kinaesthetic impressions were not localised, but were indicated by "a feeling of restlessness;" Reagent VIII also speaks of a "feeling of discomfort" with a "desire to turn."

The imagery of Reagent V involved a visual or at least a spatial element consisting of an imaged straight line or beam from the experimenter's eyes to the back of her head; and a marked kinaesthetic impression, leading to "yes" guesses given with a high degree of certainty, was tension of the eye-muscles toward this line. "Attention and eyes drawn toward line," was a frequent introspection for guesses given with a higher grade of certainty. She also has clear visual imagery of the experimenter either accompanying this kinasthetic impression, and including the "line," or of the experimenter's face turned away. The visual element gave way, in the course of her experimentation, to the kinaesthetic as a guide to the more certain guesses. Reagent VII also mentions this "line" in her visual imagery of the experimenter, and speaks of a "feeling of connection."

The kinaesthetic impressions involving restlessness, desire to turn, strain in the neck-muscles and in the eyes, were shared by other reagents who had other modes of imagery from which they made their guesses; as, V, VIII, IX.

More subtle kinaesthetic imagery was sometimes evidently of influence in determining the guess. Reagent IX "felt like answering a call of her name;" and VIII recorded a "feeling of being alone" which was a positive determinant for a "no" guess; and of a

"feeling of being criticized," or a "feeling of nearness to the experimenter," both of which yielded "yes" guesses.

(3) Inferences were sometimes drawn by the reagent from sounds of the experimenter's manipulation of apparatus or his conduct of the experiment. After shaking the dice-box, the experimenter waited until the second-hand was coincident with a five-second dial-mark before he tapped. Reagent I noticed variability in the length of this interval, and inferred that longer intervals were caused by preparing "to stare;" and he confidently gave for these cases "yes" guesses; he sought for a basis for inference when at a point in his series impressions failed to come during the interval. Other reagents noted in the pre-period a sound of movement from the rustling of clothing, and inferred that the head was being raised "to look;" when such impressions came within the interval, the reagent inferred that the experimenter was not looking. Reagent VIII "knew from her movements" the experimenter was not looking, and also inferred that "harder taps" were signals for a "yes" guess.

Even when such impressions are not used in "inferences" they may conceivably influence the guessing by being taken advantage of subconsciously. It is impossible for the experimenter to maintain perfect uniformity in his conduct of the experiment, which involves, among other things, length of the various intervals, breathing, manipulation of the dice-box, intensity and accent in tapping, slight bodily movements, etc. Great effort was made, however, to maintain uniformity, and this may in part account for the lack of an excess of R judgments.

Inferences may also be based upon hypotheses, and depend in their outcome upon subjective conditions; *e.g.*, Reagent VII inferred from internal distraction that E was not staring, or the distraction would have been overcome; and entire absence of impression was inferred to mean that none was sought to be made.

Other tendencies were also noted: "What did I answer last" influenced Reagent II, who was obviously endeavoring to keep positive and negative guesses about equally frequent. She also occasionally made up her mind, Marbe-fashion,[5] to say "yes" next time; but since the series was not voluntarily made by the experimenter, coincidence due to like tendencies of the two minds was excluded. And when she was "tired and bored" she wanted to say "no," as a general protest to further experimentation.

Conclusion.—Our conclusion, with respect to normal reagents, is (1) that the belief in "the feeling of being stared at" is quite common (shared by over half of the university students); (2) that experiment shows it to be groundless; (3) that there is an explanation supplementary to that mentioned by Titchener (nervousness, attracting attention, turning, catching the gazer) for the existence of the belief, lying chiefly in *attributing an objective validity to commonly experienced subjective impressions in the form of imagery, sensations, and impulses.* This is a tendency which, under favorable conditions, works itself out in Hallucinations and Motor Automatisms, and it seems to be a common trait in normal adults.

[5] K. Marbe, Ueber das Gedankenlesen und die Gleichförmigkeit des psychischen Geschehens. *Zeitschr. f. Psychol.*, lvi., 1910, 241-263.

PROJECTION OF THE NEGATIVE AFTER IMAGE IN THE FIELD OF THE CLOSED LIDS

By Frank Angell

In the preceding numbers of this Journal, the writer together with one of his students set forth the results of an investigation on the apparent distance of the negative after-image in the field of the closed eyes. Up to the time of printing, the writers had found no material bearing on the topic save casual references by Fechner and Hering. The fated article which, of course, had a Teutonic existence somewhere, has turned up in v. Graefe's *Archiv* for 1885 with Dr. G. Mayerhausen as sponsor and as title "Ueber die Grössenverhältnisse der Nachbilder bei geschlossenen Lidern" (*Abteilung* 2 S. 23-24). The results differ so widely from those published in the Journal as to call either for explanation or discussion or both.

Experimenting on himself, Mayerhausen finds that the negative after-image in the darkened field appears as large as the inducing object when the latter is set at a distance of two meters. In the case of the Stanford observers, three in number, unacquainted with each other's findings, the region of equality lay inside of eighteen centimeters from the cyclopean eye.

Mayerhausen's method was as follows: As inducing objects he used four white discs, 1, 2, 4, and 8 cm. in diameter respectively, and developed after-images from them at ten different distances from the eye, beginning with five meters and running down to 10 cm. When the after-image was at its maximum clearness, he opened his eyes and compared its diameter with the readings of a centimeter scale held in his hand. In this way, he made eighty readings, viz., two for each size after-image at each of the ten distances. The writer humbly submits that this is a less trustworthy procedure than that described in the Journal where the after-image with closed eyes was compared with the image projected on an adjustable screen until a region was found where the two images showed no appreciable difference. Within this region, after-images were induced and their size measured by projecting them on a ruled screen. The equating of the two forms of after-image was almost as accurate as if the images had been actual cardboard disks, successively compared. The estimation of the projected image was harder but nevertheless the size of the mean variation of the readings (from 4% to 10%) for the three observers showed that the judgments were based on actual if rough measurement.

Mayerhausen combines all his figures into a composite curve in which the abscissal distances are the distances of the inducing discs from the eye and the ordinates are the ratios of the diameters of the inducing discs to the estimated size of the corresponding after-images. The curve indicates a very rapid falling off in the size of the after-images from 10 to 50 cm., and a gradual decrease up to the farthest distance used, viz., five meters. At 30 cm. the image is more than twice as large as the disc itself; at 100 cm. it is 1.17

times as large and at 200 cm. as stated above, image and disc are equal.

Of course the obvious answer to the discrepancy of results is that the methods were different. Accordingly the next thing to be done was to try Mayerhausen's way of measuring after-images with a centimeter rule, though instead of the hand we ventured to use a tripod placed at a convenient distance. The results of these measurements are given below:

Observer *W. T. R.*, assistant in laboratory experiments in preceding investigation on after-images. May have looked for small images, but was simply given directions for repeating Mayerhausen's experiment. Each figure is the average of eight readings:

Distance of square from observer		1 m.	2 m.	3 m.	4 m.	5 m.
Estimated size (in mm.) of image of	8 cm. disc	12.6	9.8	6.7	4.8	2.8
	4 cm. disc	6.6	4.8	3.0	2.3	1.5
	2 cm. disc	3.0	2.2	1.7	1.1	1.1
	1 cm. disc	1.8	1.1	1.–	0.5	0.2

Observer *F. A.* Was observer in preceding investigation, anticipated relatively small size of images. Experimented only once with the 3 cm. and 4 cm. discs. Was usually unable to dectect the after-images from a one cm. disk placed at 3, 4 and 5 meters, and not alway when placed at 2 and 3 meters. When seen, the image was roughly estimated as somewhere near one millimeter.

Distance of 4 cm. disc from observer.	1 m.	2 m.	3 m.	4 m.	5 m.
Size of after-image in mm. (4 trials)	5.10	2.3	1.53	1.2	1 and less.

Observer *G. T.*, advanced student in Psychology, had not served in previous investigations and was unacquainted with its results. Experiments only with 1 cm. disc.

Distance of 1 cm. disc from observer.	1 m.	3 m.	5 m.
Size of after-image in mm. (5 trials)	2	1	0.5

These figures accord with the general experience of the laboratory. In the introductory laboratory course, the students are usually unable to find, at the outset, the after-image of the 1 cm. disc placed at a distance of 40 to 50 cms. as they usually look for an image which approximates the object in size instead of the minute figure of 2 or 3 mms. which they finally detect.

It is at this point that Mayerhausen's figures are wholly inexplicable to the writer. It is conceivable that with the larger discs one can imagine a greater or less distance of projection and so perhaps a greater or less size. But the images from a 1 cm. disc placed at 4 to 5 meters are simply minute gray points. If one uses a black disc on a white ground the after-image will be a point of faint light, not unlike a star of small magnitude. But Mayerhausen estimates the size of the after-image from the 1 cm. disc placed at a distance of 5 meters as from 4 to 6 mm. which is larger than our estimations for a 4 cm. disc acting from a distance of 2 meters. At the outset, however, Mayerhausen instances an experiment which he seems to consider eliminates the factor of distance in explaining the size of projected after-images. This crucial experiment he describes as follows: "If, in the after-image experiment, I fixate a distant wall, and while fixating, slowly push in a sheet of paper between my eyes and the wall (say at a distance of 30 cm.), the image now appearing

on the paper will retain exactly the same size which it formerly had on the wall." "Daraus geht zur Genüge hervor," he continues, "dass bei gleich bleibender Fixation es für die Grösse eines Nachbildes absolut gleichgültig ist, wo die Projectionsfläche liegt." This is surely a feeble lever for overturning so well-grounded a structure as the theory of the relation of size and distance of projection of after-images. For it is evident that what we have in this experiment is a blending of the wall and the paper to a common surface which is projected to the distance of either wall or paper. If the after-images of two discs say of 5 cm. diameter, placed 5 or 6 cms. apart, are projected on the wall and then the paper is pushed slowly in until its edge falls between the images of the disc, one may get two sizes of disc images, one corresponding to the distance of the wall, the other to the paper. In fact, Mayerhausen's 'blend' experiment is evidence against his argument, for the sizes of the after-images will correspond either to the supposed distance of the wall or of the paper, and to no other distance.

As distance of projection is not for Mayerhausen the main factor in determining size, convergence must play the chief rôle, and accordingly he concludes that equality of after-image and inducing object at the distance of two meters, is owing to the intersection of the optic axes at this distance. When the eyes are closed, he assumes that convergence at 2 meters forms, at first, a sort of state of provisional equilibrium, which passes over into parallelism of the axes in sleep (*op. cit.*, S. 25). He further assumes that when the object fixed is at a distance, say, of one meter, and the eyes are then closed to develop the after-image, the stronger convergence does not pass over into the state of equilibrium, but a lesser relaxation follows which in his case multiplies the size of the object by 1.17, though, of course, the retinal image is doubled in diameter.

It does not appear that Mayerhausen investigated the movements of the closed eyes when developing after-images. Assuming that the distance of projection depends on convergence, he concludes that the motions must have taken place to produce the results indicated by his figures.

But it certainly is a matter of no great difficulty to acquire the power of converging the eyes at will when closed or when in a dark room and when the trial is made it will not be found to have much influence on the size of the after-image. Hering says that he can alternately change parallelism of the optic axes for the greatest possible convergence without changing the apparent size of the after-image in the field of the closed eyes,[1] and the writer has not been able to note any regular change in the size of the after-image when repeating the experiment. It even makes no difference to the apparent size and position of the after-image if one eye is displaced by pressing on the bulb, a condition which, of course, with open eyes produces double-images.

It is unfortunate that, in this and other psychological investigations, observers obtaining such different results from the same problem can not come together and compare methods of work. The discrepancy is probably owing to some unnoted difference in the details of the operations which joint investigations could bring to light,—to the advancement of knowledge and the saving of printers' ink.

[1] E. Hering, *Beiträge zur Physiologie*, I, 138.

PROFESSOR MARTIN ON THE PERKY EXPERIMENTS

I have received the following letter from Professor Martin:

STANFORD UNIVERSITY, CAL., July 21, 1913.

DEAR PROFESSOR TITCHENER,

In mentioning only the three reagents who took part in Series IIb and IIc, in your recent discussion (this JOURNAL, xxiv, 124) of the investigation regarding the difference between memory and imagination images, found in my work *Die Projektionsmethode und die Lokalisation visueller und anderer Vorstellungsbilder*, you certainly give an incorrect impression as to the data upon which I base my impression regarding the Perky investigation.

Series II, IIa, IIb and IIc were all made with the purpose of getting at the difference between the images of memory and imagination. In Series II and IIa in which the results (see p. 78) as well as those of IIb and IIc do not confirm those of Perky and in which I naturally place more confidence, on account of the wider experience of the reagents, not alone as *Versuchspersonen* but as *Versuchsleiter*, the following *ten* members of the Psychological Institute of Bonn University acted as observers (see p. 8): *die Doktoren Baumgarten, Behn, Bühler, Morisse, Rieffert, Schanoff, Professor Girgensohn; die Herren Kemp, Köhler und Silberstein.*

I shall, of course, be very glad if you will publish this letter in THE AMERICAN JOURNAL OF PSYCHOLOGY.

Very sincerely yours,

LILLIEN J. MARTIN.

I should be sorry to give an incorrect impression of Professor Martin's work,—though I can hardly have done so to a careful reader,—and I therefore add a word of explanation. Series II and IIa were completed before Professor Martin became acquainted with the Perky experiments, and were made by her own method of projection (*Z.*, lxi., 1912, 398). Series IIb and IIc were made with direct reference to Perky, under conditions which, in Professor Martin's judgment, approximated as closely as possible to those of Perky (*ibid.*). I open my discussion with the words: "I do not intend to offer here any general criticism of Professor Martin's recent work on imaginal complexes . . . . I defer . . . . discussion until further experimental evidence, for or against, is forthcoming. It is my present purpose to examine only a small part of her published work . . . . undertaken as a test of certain results obtained by Perky in my laboratory." And I say later: "We are now taking up the whole problem afresh, with special reference to Professor Martin's method of projection."

In the letter printed above Professor Martin *has not said a word in reply to my criticisms of Series IIb and IIc:* I may take it, then, that she admits the justice of these criticisms. She confines herself to a mention of Series II and IIa, that is, of experiments *performed by a method which I expressly declined to criticise until I had experimental data at command.*

E. B. TITCHENER.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF VASSAR COLLEGE

XXII. The Effect of the Interval Between Repetitions on the Speed of Learning a Series of Movements.

By Mildred Browning, Dorothy E. Brown, and M. F. Washburn

The fact was discovered by Ebbinghaus and thoroughly established by Jost that verbal material is learned with fewer repetitions if an interval is allowed to elapse between successive repetitions; if, that is, the repetitions are 'distributed' rather than crowded together. Now such a process of learning series of words or nonsense syllables involves two factors which may or may not obey the same laws. One factor is the establishment of associative dispositions, whereby one member of the series recalls the idea of other members. The other factor is the establishment of a habit of movement on the part of the articulatory muscles. These factors may be briefly called the associative and the motor factors in learning, and one of the most fundamental problems in the psychology of learning concerns their mutual relations.

In the present study the intention was to set a learning task in which the motor factor should play a larger rôle as compared with the associative factor than is the case in the learning of verbal material, and to see whether the law of distributed repetitions held good for such a task. If we wish to acquire a habit of movement involving the larger muscles of the body, shall we do better if we go through the movement a number of times in immediate succession, or if we allow an interval between performances?

Our problem was constructed as follows. On a sheet of cardboard a rectangle was drawn about thirty inches long by eighteen inches high. This rectangle was divided into eight compartments by lines drawn upon it, four compartments in the upper row and four in the lower row. Each compartment had a single letter printed in it, differing from the letters in the other compartments. The cardboard thus prepared was laid upon a table before the observer, whose eyes were closed. A pack of cards was placed in her hand. This pack was composed of cards, each one carrying a letter corresponding to one of the eight letters of the cardboard diagram. There were sixteen cards in the pack, hence, each letter appeared on two cards. The cards were numbered on the back. The observer, always with eyes closed, sat holding the pack, her hands above a mark on the table against which the center of the diagram rested. The experimenter took hold of her right wrist, and moved her right hand, to the time of a metronome, in such a way as to sort out the pack of cards correctly on the diagram, putting each card in the compartment bearing the letter which corresponded to the letter on the card. After each movement the observer's hand was brought back

to the mark in front of the center of the diagram. Thus the observer's hand was guided by the experimenter through a series of sixteen movements. The exact sequence of these movements had been carefully planned and was indicated by the numbers on the back of the cards, while the letters on the front guided the experimenter in the sorting. The observer, of course, keeping her eyes closed, knew nothing of letters or numbers; she simply felt herself being put through a series of movements, and formed some visual images of the position of her hand. She was now caused to go through this same performance repeatedly until she could sort the cards correctly without having the experimenter guide her at all. The experimenter, while still keeping her hand on the observer's wrist, gradually lessened the amount of her control as the observer came to 'know' the movements, until finally a correct sorting was made without contact from the experimenter. A record was kept of the number of repetitions required before this point in the learning was reached.

Each observer learned two such series of sixteen movements. With one series, the repetitions followed immediately one upon another without interval. To enable the repetitions to be made in such rapid succession, thirty packs of cards, exactly similar in arrangement, were provided, so that a fresh pack was put into the observer's hand as soon as she had finished a sorting, the experimenter merely sweeping the pack previously used from the board without stopping to rearrange it. The movement series was always learned in fewer than thirty sortings. A second series was learned with the repetitions at intervals of one minute. This allowed time for the experimenter to pick up and rearrange the cards between sortings. During the one-minute interval the observer's attention was distracted to prevent recall of the movements.

It is evident that one such series of sixteen movements may be a good deal easier to learn than another. If, for instance, the movements come in a sequence that can be easily visualized, the series will be readily learned. In 1911-1912 Miss Mildred Browning made under Professor Washburn's direction a set of experiments on the plan described above, using always the same series of movements, which we may call Series I, for the 'no interval' experiments, and another series, II, for the 'one minute interval' experiments. Great care was taken in the construction of these series to make them of equal difficulty. The results showed that the one minute interval produced decidedly quicker learning than the absence of interval. Since, however, it was always possible that Series II was easier than Series I, Miss Brown in 1912-1913 repeated the experiments, using two new series, A and B. No particular care was taken to make these two of equal difficulty, but in half the experiments Series A was the 'no interval' series and Series B the 'one minute interval' series, while in the other half the conditions were reversed. We also took precautions to eliminate the effect of practice, by making with half the observers experiments in the order 'no interval—one minute interval,' and with the other half experiments in the reverse order. There were twenty observers, of whom twelve had had considerable practice in psychological experimentation. Each observer learned two series. It will be seen that the special practice conditions were almost perfectly uniform, a condition which is not often realized in so small a number of experiments.

The results are stated in the following table. The letters in parenthesis after the numbers are the observers' initials. 'I' means that the series was the first one learned, 'II' that it was the second.

Series A.

With Interval of 1′		Without Interval	
I.	II.	I.	II.
13(Bu)	5(T)	8(R)	8(B)
11(Q)	11(H)	4(C)	7(P)
4(Wi)	4(Su)	17(S)	6(Th)
5(Wa)	13(Ro)	11(Mc)	7(A)
5(E)	5(L)	6(Ba)	12(Sa)
Av. 7.6	*Av. 7.6*	*Av. 9.2*	*Av. 8*

Series B.

With Interval of 1′		Without Interval	
I.	II.	I.	II.
11(B)	19(R)	13(T)	19(Bu)
12(P)	3(C)	30(H)	24(Q)
11(Th)	7(S)	13(Su)	19(Wi)
13(A)	18(Mc)	28(Ro)	16(Wa)
11(Sa)	10(Ba)	26(L)	23(E)
Av. 11.6	*Av. 11.4*	*Av. 22*	*Av. 20.2*

It appears from these results (1) that Series B was harder to learn than Series A, and (2) that an interval of one minute is more favorable to learning of this kind than no interval between repetitions. The average number of repetitions in all the 'no interval' experiments was 14.8; the average number in all the 'one minute interval' experiments was 9.6. It further appears (3) that the advantage of the one minute interval over no interval is more marked for the more difficult Series B. The difference between the averages for Series A was only one repetition, while for Series B it was 9.5 repetitions. While there was a good deal of variation in the results from individuals, it seems (4) that on the whole fewer rather than more repetitions were needed to learn a series when another series had been learned just before. The average number of repetitions tends to be a little smaller when the series is the second one learned. This indicates that practice was stronger than any tendency to interference of the older habit with the newer one.

Of course the learning of such series of movements involves both associative and motor factors. In the learning of the easy Series A, particularly, our observers reported that visual imagery helped them. It would not be proper to say that while Jost's experiments proved the value of distributed repetitions in the formation of associative dispositions, ours suggest a similar law for the formation of habits of movement. Jost's material involved motor as well as associative processes and ours involved associative as well as motor processes. We can, however, claim to have shown the probability that a certain degree of distribution of repetitions is favorable to learning in a case where the motor habits are not habits of articulation. And since any habit of movement would involve both associative and motor factors, we may say that there are indications of the validity of the law of distributed repetitions for habit formation in general. The significance of the fact that the advantage of the interval is less marked in the case of the easier series is doubtful. An easy series of sixteen movements, such as the one used here, involves in

its learning a good deal of visualization. The pattern of the movements is acquired and held, at least for the easiest parts of the series, as a whole simultaneously present in consciousness. Succession in time, on the other hand, is the special characteristic of processes with a strong motor element. It is possible that the law of distributed repetitions is a motor rather than a purely associative law, and that its validity in the learning of verbal material is due to the motor or articulatory factors in such learning.

XXIII. A Suggested Coefficient of Affective Sensitiveness

By Helen Clark, Neida Quackenbush, and M. F. Washburn

It is a curious fact that in experimental studies of individual psychology no attention, so far as we are aware, has been paid to the characteristic which we shall call affective sensitiveness. By this term we mean a tendency to strong affective reactions, whether of pleasantness or unpleasantness. Evidently affective sensitiveness may be general or special. If it is general, and marked, the individual possessing it will tend to strong likes and dislikes whatever the nature of the material presented for affective reaction. If it is special, he will show sensitiveness to one kind of material and relative indifference to another kind. Hitherto, the only consideration has been whether an individual liked one kind of material and disliked another: our problem is to study his tendency to be strongly affected, in either direction, by one kind and to be left indifferent by another.

In attempting to find a means of measuring this character of affective sensitiveness, we proceeded in the following way. Forty pieces 2.9 cm. square of colored paper, chosen at random from the series of Bradley colors, were numbered on the back. A series of forty nonsense syllables, each composed of an initial vowel and a final consonant, was prepared and each syllable was numbered. The observer was then given the following instructions: "You will be shown successively five colors; then you will have pronounced to you five nonsense syllables; then you will be shown five colors, and so on until you have seen forty colors and heard forty syllables. As you look at each color, you are to express your judgment of its pleasantness or unpleasantness by speaking one of the numbers from 1 to 7, 1 meaning very unpleasant, 2 moderately unpleasant, 3 slightly unpleasant, 4 indifferent, 5 slightly pleasant, 6 moderately pleasant, 7 very pleasant. As you hear each syllable pronounced, you are to judge its pleasantness or unpleasantness in the same way. You will be given a 'Ready' signal before each color is shown and before each syllable is pronounced. Your eyes are to be closed except when you hear the signal 'Now,' when you are to open them and look at a color."

The colors were laid one at a time on a sheet of white paper on the table before the observer, who reported her judgment at once. The syllables were pronounced with as nearly as possible the same force and distinctness. The same order of colors and syllables was always followed. At the end of an entire series free introspective comments were made by the observer.

The results were treated in the following way. For each observer, we counted the number of 'indifferent' judgments made upon the colors, and also the sum of the numbers of 'very pleasant' and 'very unpleasant' judgments. We then divided the number of 'indifferent' or 4 judgments by the number of 'very pleasant' or 7 judgments plus the number of 'very unpleasant' or 1 judgments. The quotient was a number which varied inversely with the affective sensitiveness of the observer to colors: it was larger the more indifferent the observer was to this kind of material. We could just as well have inverted the fraction and obtained a number which would vary directly with affective sensitiveness. Either number furnishes a good index of this particular character. Of course if either the numerator or denominator happens to be zero, we can hardly conclude that the affective sensitiveness is infinitely great or small, but where the numbers have a finite value the index would seem to be roughly serviceable. A similar index was reckoned for each observer's judgments on the nonsense syllables.

Those observers for whom the ratio of indifferent judgments divided by extreme judgments was 1.5 or more were rated as decidedly indifferent to the kind of material used. Those for whom the ratio was .5 or less were ranked as decidedly sensitive. If an observer's ratio fell between these extremes (which were not, of course, held to with mathematical rigidity), she was classed as fairly sensitive.

There were seventy-seven young women observers. Among them, the largest sub-group was formed by those who were very sensitive to both the colors and the sounds. This was characteristic of twenty observers. Next in size came the group who were very indifferent to both: of these there were fifteen. There were thirteen who were very sensitive to colors and fairly sensitive to sounds; eight who were very sensitive to colors and very indifferent to sounds, seven who were fairly sensitive both to colors and to sounds, five who were fairly sensitive to colors and very sensitive to sounds, five who were fairly sensitive to colors and very indifferent to sounds, two who were very indifferent to colors and very sensitive to sounds, and two who were very indifferent to colors and fairly sensitive to sounds.

From these figures, it is clear in the first place that colors provoke a strong affective reaction in more observers than articulate sounds do, under the conditions of our experiment. The total number of observers who were very sensitive to colors was forty-nine; the total number who were very sensitive to sounds was twenty-seven. The total number who were very indifferent to colors was nineteen; the total number who were very indifferent to sounds was twenty-eight.

Secondly, it appears that there is a tendency for observers who are either very sensitive or very indifferent to one kind of material, to have the same affective attitude toward the other kind. The two largest groups were those of persons who showed great sensitiveness to both colors and sounds and of persons who manifested great indifference to both. There is a greater probability that an individual will display general affective sensitiveness, if we may call it general when we have used only two kinds of material to test it, than that he will show specialized sensitiveness to either colors or sounds. It must be remembered that the general situation is a part of the source of the affective reaction. There are probably people who cannot get any strong affective experience in the constrained and artificial surroundings of the experiment, while others are less embar-

rassed by their environment. Since the situation is practically the same with both kinds of material, it probably tends to make the affective response for colors similar in intensity to that for sounds in the case of many observers.

Thirdly, there exists a small but interesting group of observers who are strongly stirred affectively by one kind of material and left markedly indifferent by the other kind. As we should expect, there was more specialized sensitiveness to colors than to sounds: eight persons were very sensitve to colors and very indifferent to sounds, while in the case of two only was the relation reversed.

What makes an observer give strong affective reactions to one kind of material and weak ones to another kind? Has this characteristic anything to do with imagery? We should not expect that the frequent use of a certain kind of imagery, or the capacity to experience it with especial vividness, would produce any especial affective sensitiveness to sense-impressions of the corresponding modality. We do not need to recall color imagery in order to find a color pleasant or unpleasant. On the other hand, the reverse causal relation might very likely exist. A person who had strong likes and dislikes for the sense-impressions of a given modality would naturally tend to give more attention to them, and this might result in more frequent and more vivid imagery belonging to the modality in question.

We must leave for further investigation the problem of the characteristics which are correlated with specialized affective sensitiveness. So far as our results go, they indicate an entire lack of correlation with either the type of imagery most useful in memorizing, or the type occurring most readily and vividly as an illustrative accompaniment to reading. All our observers who showed specialized sensitiveness, as well as some who were very sensitive and some who were very indifferent to both kinds of material, were subjected to the following tests from Fernald's "The Diagnosis of Mental Imagery" (Psych. Monographs, vol. 14, no. 1): the rhyming tests (page 28), the similar spelling tests (page 28), the test for memory of words similar in sound but spelled differently, and that for auditory memory of words spelled alike but differing in sound (page 29), and the reading tests for illustrative imagery (pages 139-143). In each of these tests, some of the observers who did best were especially sensitive affectively to the corresponding kind of sense impression, while others who did equally well were especially indifferent.

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Note.—1901 (1) *Gustav Theodor Fechner* and 1909 (5) *Festrede* have been transferred, apparently without change save that of title, from W. Engelmann to A. Kröner.

BOOK REVIEWS

The Dynamic Foundation of Knowledge. ALEXANDER PHILIP. London, Kegan Paul, French, Trübner & Co.; New York, E. P. Dutton & Co., 1913. pp. 318.

The thesis which the author of this volume endeavors to maintain and to elaborate is that the world of sensible experience is to be interpreted in terms of a potency or potencies of which sensible objects are the transmutations. The key to our philosophical problems is to be sought neither in sense nor in thought, but in our own individual activity. Taking this activity as our starting point, we are enabled to get beyond our conscious selves, and yet we do not transcend experience. The Real can be found only in "the exertional element of our life, and that element being also that which interacts with and which participates in our environment, seems obviously the element for or by which we derive the knowledge of the independently real. In our exertional activity we are linked and intermingled with the dynamic system which constitutes our environment. Community of knowledge is rendered possible by and only by this participation. When the phenomena of sense can be accounted for as transmutations of a real energy or potency, then and only then can their appearance be explained by a theory consonant with the requirements of an intellectual metaphysic" (p. 64).

On this basis the proper procedure is a preliminary study of the body. The dynamic activities pertaining to the body are discovered by us through the stresses resulting from the limitations and opposition of the environment. These stresses are reported by the nerves to the sensorium, which is the common point of origin of the nerves of the special senses as well as the ultimate terminus of the afferent and excitant nerves of touch and motion. If the sensible impression is not transmitted beyond the sensorium, it is represented merely by a sensation. But the cerebrum may be stimulated, with the result that ideative cognition of the external is induced, the sensation being the material upon which it operates. On the basis of this distinction the author recognizes two kinds of activity, motor activity and the activity of thought. The former is conditioned by space, and hence the axioms of geometry have an *a priori* warrant. The activity of thought is not thus conditioned and hence it is bound by no *a priori* law save that of contradiction.

These axioms or *a priori* conditions, however, are not to be conceived in the manner of Kant, but are conditions laid down by the nature of power, potency or energy, which is the true and fundamental postulate of philosophy. This power, when it takes the form of movement, operates under the appearance of tridimensional space; when it takes the form of thought it operates in accordance with the law of contradiction. The rather obvious inference that if space is merely an appearance, contradiction must likewise be merely appearance seems to be left unnoticed by the author. Nor is he willing to concede that the limitation of thought to the material of sense rules

out the concept of power or potency. "Cerebral activity is adapted to represent, though it is not confined to representing, the dynamic process; and in such representative activity the object is not sensation but motor activity. Thoughts, or ideas, as Plato called them, when they represent the process of reality, do not do so by reproducing in fainter form the fleeting sensations which are the mere accompaniments of the obstruction of motor activity. What cerebral activity does is to reproduce or rather represent the dynamic activity in which sensation arises" (p. 187).

The merit of the volume, as it seems to the reviewer, lies in its recognition that the traditional apparatus of sensation and thought is inadequate. Hence the attempt is made to find in power or potency a category that will remove the difficulties. But the author's account of the individual experience which arises in response to the obstruction of motor activity lands us once more in the time-worn categories of sensation and thought, and confronts us anew with the familiar puzzle of representationism. The things of sensible experience *represent* "the result or supposed result of action;" and similarly the activity of thought represents the dynamic activity in which sensation arises. "The mariner observes his position at noon. It is a static fact, but it is determined by dynamic changes; and is not its object to determine the course of his vessel? What is true of the visible is equally true of the tangible, of the audible, and of the whole sensible world. All tactile resistances in some way represent dynamic action" (p. 67). This *might*, of course, be interpreted in the pragmatic sense that objects of sense embody or reflect an adjustment that is completed and that their *meaning* is exhausted in adjustments which are to be made at a future time. But the author does not follow this road. The potency with which he deals is a reality or entity in an absolutistic sense, and like every other ultimate it is under no particular obligation to make itself intelligible.

How the fact of activity becomes a matter of knowledge is not made very clear, even though a considerable portion of the chapter on *activity* is devoted to this question. It is obviously not a fact of sense-perception, for if it were, there would be no justification for the condemnation of sense-experience as purely phenomenal. But neither is it an intuition. The question is left unanswered, unless we regard as answers certain *nichts sagende* statements such as the following: "It is by and in the obstructions of my exertional activity that its forms are defined. Sensation seems to be the medium by which both Discourse and Exertion are rendered knowable" (p. 25).

Waiving this point, however, we find further perplexities in store for us when we attempt to consider the nature of this potency. It is not merely the absence of description but the absence of coherence that is likely to trouble the reader. The author at the start makes appeal to the prevalence of universal change, apparently on the basis of observed fact, and concludes from this that we do not observe the real but only the transmutations of the real. Where observation fails to establish this change, physics and chemistry help out with their description of the processes involved in perception and in the fact perceived. Having thus compelled observation to commit suicide by means of its own category of change, the author proceeds to include space and time among the changes which the real undergoes. We are thus—to paraphrase a recent writer—treated to the spectacle of seeing a man carry a variety of things into a house and finally carry the house itself in. "The ascertained laws of energetic

transmutation involve the dynamical relativity of Space and Time, which are quantifying ratios, but not at all the categorical continent of Experience. The antinomies of temporal succession, the dilemmas of Zeno, disappear when we envisage the fact that the energetic transmutation is in essence timeless and inextensive" (p. 196).

It does not seem worth while to follow out in further detail the windings of the argument. The absence of any serious attempt to define the fundamental concepts of the book—sensation, representation, knowledge, independence, and change or transmutation—gives little promise that any genuine contribution is to be expected. And it may be added that a better understanding of the present-day discussions which the author dismisses so complacently with a flourish of rhetorical phrases is more to be desired than additions to the long list of sins that are committed in the name of philosophy.

University of Illinois. B. H. Bode.

The Learning Process. By S. S. Colvin, Ph.D. New York, the Macmillan Co., 1911. pp. xxv, 336. Price, $1.25, net.

In order to be of value from the point of view of the teacher, educational psychology must be neither so speculative and general that it offers no better aid in the solution of practical problems than does common sense, nor so abstract and technical that its facts have no relation to the everyday work of teaching.

Unlike many books on the subject, the *Learning Process* can escape both of these criticisms. "The point of view," says the author, "is a thoroughgoing functionalism and pragmatism." There are no long discussions of the relation of body to mind, the structure of the nervous system, the nature of mind, and so on. After a general consideration of the fundamental elements of the learning process, the author goes directly to the concrete facts. The chapters on habit, sensation and perception, imagination, memory, association, and the transfer of training are clear, condensed accounts, drawn chiefly from experimental works, with frequent summaries and constant interpretation to hold the facts together. Each chapter has a direct and evident bearing on the chapter on application which follows it. These latter chapters are perhaps the distinguishing feature of the book. The recommendations and rules contained in them are many, and they are definite, concrete, and detailed. The chapters on transfer of training, for example, are followed by the application of the given principles to such questions as the disciplinary value of the various studies, the elective system, pure *versus* applied science, the importance of ideas and attitudes in the formation of habits; the chapter on memory, by application to questions of learning by wholes or by parts, distributed learning, 'bunching' the school program, short and unrelated courses, the value of the recitation and of examinations, modes of presentation, aids in learning, etc.

The chapters on attention and interest and on the higher thought processes, and a part of the discussion of reflex action and instinct, are rather more classificatory, theoretical, or formal than the rest, and the generalizations in them find less evident and less extended application. It seems probable that some of these generalizations cannot be specifically applied; if so, a portion of the material might have been omitted (for example, the discussion of attributive clearness, and the structural and logical account of concept and judgment). The statement (p. 140) that "Ebbinghaus attempted to exclude all associative factors in his learning," and that "he really

tested not the loss of memory as such, but merely the fading of the memory after-image" is surely inaccurate.

The book as a whole is a decided step in the right direction—away from generality and technicality, and towards concrete facts and their specific application. It is probably the most practical text-book of educational psychology that has yet appeared. W. S. FOSTER.

An Elementary Study of the Brain, Based on the Dissection of the Brain of the Sheep. By EBEN W. FISKE, A.M., M.D. Illustrated with photographs and diagrams by the author. New York, The Macmillan Co., 1913. pp. vi, 133. Price, $1.25, net.

The book is an elementary laboratory manual for a special course in biology. The first two chapters orient the student in the comparative (phylogenetic and ontogenetic) anatomy of the brain. The third to ninth chapters contain directions for the dissection of the sheep's brain and descriptions of the structures to be studied. Physiological and psychological aspects of brain-formations find mention in various places, but they receive greatest attention in the summary, chapter X. Here we find a discussion of the central representation of the senses in the lower animals and man; the direct and indirect paths of conduction from the sense-organs to and through the cortex are traced; and the human brain is broadly interpreted in terms of its history and functions. One can only wish the chapter were less brief and schematic.

The author follows a good rule in giving the etymology of the technical terms which beset the beginner in brain-anatomy and brain-physiology. Unfortunately, however, he has not submitted his derivations to a philologist, with the result that he often falls into error. *Protoplasm* should be derived from Gk. *proto-*, first, and *plasma*, something formed. *Ontogenetic* comes from Gk. *on* (*ont-*), being, and *genetikos* adj. of *genesis*, generation. There is no Latin word *bi* for two, or *quad* for four; and the forms *bigeminus*, *quadrigeminus* are themselves Latin. The Gk. infinitive *phyein* is transitive; and the forms *hypophysis*, undergrowth and *epiphysis*, excrescence are already Greek. *Pellucidus* is a Latin adjective. *Cineria* should be *cinerea*. *Arachnoid* represents the Gk. *arachnoeides*, not *arachne*. *Chiasma* is itself a Gk. word, and there is no Gk. verb *chiozein*. The Gk. word *eidos* does not mean *like*, and the word *arche* does not mean first. Instances of this sort could be multiplied, and show a carelessness that is sadly out of place in a scientific manual.

The simplicity and clearness with which the difficult subject is presented are worthy of praise. The photographs and diagrams are numerous and excellent. There is a bibliography of twelve titles. W. S. FOSTER.

Palaeolithic Man and Terramara Settlements in Europe. By R. MUNRO. Being the Munro Lectures in Anthropology and Prehistoric Archaeology in connection with the University of Edinburgh, delivered February and March, 1912. New York, The Macmillan Co., 1912. pp. xxiv-507.

The first and larger portion of this volume (pp. 1-287) deals with the material remains, and the culture and civilization, of the palaeolithic races of Europe. The object of the Munro foundation is "to popularize prehistoric methods and research, and to stimulate the inquiring faculties of intelligent persons to pursue the subject on

account of its inherent interest to the civilized races of the present day." The style of the lectures is therefore popular, and the illustrations are lavish. We begin with introductory chapters on the "epochs" of the Stone Age, on the methods of cave-exploration, and on chronological problems; then follow discussions of the fossil remains of palaeolithic man found in the various European countries, with a special chapter on *Pithecanthropus erectus*. The sketch of culture and civilization deals, in the main, with the artistic phase of prehistory, though we read incidentally of tools, weapons, clothing, cooking, etc.; other sides of folk-psychology, such as the question of the domestication of animals, the evidence of magical and religious practices, and so forth, are left for later lecturers. In a chapter on the transition from the palaeolithic to the neolithic status, the author makes less than certain other authorities have done of the completeness of the change: "I am not aware," he says, "of any evidence which actually negatives the idea that the Quaternary men of Europe survived till the arrival of the Neolithic tribes, and that both races continued to live amicably in the same neighborhood and ultimately amalgamated with each other."

The lectures furnish a very useful compendium of information,—though the reader will wish, at times, for a more incisive handling of detail, and for the banishment of references from text to footnote. The material is brought up to date: it is not the author's fault that no account is given of the Piltdown skull, probably the most important discovery of the kind hitherto made in England (*Nature*, December 19, 1912).

The remainder of the book (pp. 291-476) has a more restricted topic: it deals with the *terremare* or "marl beds," first found in the valley of the Po, which proved on examination to be the remains of villages of the Bronze Age, groups of huts supported on piles and fortified by moat and earthern rampart. The *terramaricoli* were apparently lake-dwellers who had crossed the Po and carried the habit of pile-construction with them. Dr. Munro traces the relation of these invaders to the Neolithic hut-builders and cave-dwellers of the same region. His view is that the original invasion occurred from the Danubian valley, by way of Croatia, Carinthia and N. E. Italy; that the newcomers reached the Po valley in the period of transition from Stone to Bronze (certain settlements in the Lake of Garda were inhabited up to the beginning of the Iron Age); that they moved westwards along the left bank of the Po, and crossed it, to found *terremare*, near Vadiana; and that presently, moving south, they took possession of the native villages and occupied their hut-shelters, thus giving up the *terramara* habit. A concluding chapter describes structures analogous to *terremare* in other European countries.

These lectures, which were also delivered on the Dalrymple foundation at the University of Glasgow, are again abundantly illustrated, and are accompanied by a complete bibliography.

The Physical Basis of Music. By A. Wood. Cambridge, University Press; New York, G. Putnam's Sons, 1913. pp. iv-163. Cambridge Manuals of Science and Literature No. 55. Price, 40c., net.

This little book gives a clear account of the elementary physics of sound, so far as concerns the production of musical tones. For psychology it relies wholly upon Helmholtz' *Sensations of Tone*, and must therefore be pronounced out of date.

Qu'est-ce que le raisonnement? Par EUGENIO RIGNANO. Scientia, XIII., 1913. pp. 30-57.

In this article the author confines himself to the analysis of certain of the simplest and most common forms of reasoning, reserving for future contributions the study of the evolution of reasoning and the development of its higher forms.

From the analytical study of a series of examples he reaches the conclusion that reasoning consists in the imagined execution of a series of observations or experiences which might have actually been carried out,—e. g., the author, not finding his umbrella in its usual place, thinks momentarily that he may have left it in one of the places where he was on the previous day; but he then reasons that as he did not change his clothing, although it was raining heavily when he came in, this could not have been the case. Here the reasoning is simply the mental representation of a series of experiences, which might have occurred.

In the somewhat different type of geometrical reasoning as, for example, the proof that the sum of the interior angles of a triangle equals two right angles, the actual process of placing the angles in juxtaposition can be carried out by cutting a paper triangle, but is valid only for the particular triangle. Precisely this is accomplished by the imagination. We simply transfer in thought the angles to a common vertex and mentally apply a previous experience to the situation and in making use of an empirical result already obtained we obtain a general value, not obtainable by the actual experiment.

From numerous examples thus analysed, it appears that reasoning is nothing more than a series of representations in thought of operations or experiences either actual or possible.

If the true nature of reasoning is not clearly comprehended, the perfect agreement of the results of the logical process with results actually observed may cause a feeling of surprise and wonder, but if we recognize that the logical process, reasoning, is only a series of experiences, which are all, theoretically at least, susceptible of being carried out but which, in order to save time and energy, are simply limited to the thought, the wonder ceases.

In other words, the intermediary results of all reasoning, even that which develops by means of the most complicated symbolism (to be discussed by the author in a forthcoming paper), have all a concrete symbolism, i. e., they represent the respective empirical results of the different phases which succeed each other in the series of operations or thought experiences.

From this nature of reasoning arise certain advantages and disadvantages. It is evident that in reasoning there is an enormous economy of time and energy as compared with actual carrying out of the experiences.

There are moreover an infinite number of experiences, which though theoretically possible could never be carried out in practice. Reasoning can thus accomplish a far greater number of experiences than would be materially possible and, moreover, can give a more general result as e. g. in the solution of the problem of the value of the interior angles of a triangle.

But there are also disadvantages, which arise from the risk of error which inevitably exists from the very nature of the reasoning process. When the complexity of the process passes a certain limit in consequence of the multiplicity of the experiences mentally accomplished, then it becomes impossible to follow in thought all the factors

and their reciprocal effects which are involved and consequently one or more of them is forgotten. Moreover since we are compelled to use symbols, verbal or otherwise, in our thinking, a source of error is introduced which renders absolute confidence in purely mental results impossible.

But on the other hand, the sterility of pure reasoning affirmed by some authors and the assertion that nothing is contained in the conclusion which was not in the premises is untrue, as may readily be shown by the many new facts in science discovered purely by reasoning. The new combination in the imagination of experiences already known leads to the discovery of absolutely new results, to a new truth which is contained in the combination of facts, but which exists in neither of the facts taken singly. It is this new mental vision created by imagination which constitutes the new fact, the conclusion. But it is also true that though reasoning is quicker and therefore more productive, actual experiment may furnish better conditions for the discovery of new facts, because of the insufficiency of the imagination and because in some cases the observation of all that actually happens gives adequate data.

The fecundity of reasoning depends upon the fact that the imagination is not only reproductive but also productive, i. e., it may combine elements given by experience in a manner entirely different from anything already observed in the past.

For these new combinations the affective intensity directed toward the end to be attained is of supreme importance. An analysis of this dynamic aspect of reasoning shows that it consists in an interest which operates for the exclusion not only of all other affectivities but of memories connected with them. It also directly evokes all memories, facts, experiences, and knowledge associated with the affective tendency which is active throughout the whole reasoning process. But this affective evocation is not sufficient in all, especially in new cases, which must proceed by the method of selecting, from the multiplicity of acts imagined, those particular ones which are suited to the end to be attained. It is precisely this triple form of activity, i. e., exclusion, evocation and selection according to the affective tendency which is the essence of teleological thinking.

Simple association which suffices to explain the evocation and succession of ideas is not adequate to explain the directed association which constitutes reasoning. There is needed in addition the affectivity for the end in order to maintain coherence during a long process of reasoning.

There is also present in the reasoning process a secondary affective tendency which consists in the fear of omitting some of the possible actions and reactions to which the object under consideration might hypothetically be subjected, and this exerts an influence on the process of recall. Illogical thinking is, in fact, due to the forgetting or displacement of some factor necessary to the correct result. This phase of thinking the author proposes to discuss in a future paper on the pathology of reasoning. THEODATE L. SMITH.

In the Shadow of the Bush. By P. A. TALBOT. New York, G. H. Doran Co.; London, W. Heinemann. 1912. pp. xiv., 500. Price $5 net.

This book reports the nature and nurture of the Ekoi, a forest or 'bush' people of Southern Nigeria and the Cameroons. The Ekoi

are mainly of Bantu stock, and are found to the number of some 20,000 in and about the Oban district; across the German border they number from 6,000 to 17,000, according as certain tribes are or are not included in the count. The author, who is an official of the Nigerian Political Service, has traveled some 1,700 miles annually in the district since he entered upon his duties in 1907, and has made notes of things as he came across them. The result is a book of real interest and value, but a book which has also the defects of its origin. It is cast in the form of an itinerary, with continual interruption by folk-tales, and with special chapters devoted to religion, magic, government, etc.; so that the reader who tries to hold the narrative as a whole is reminded of *The Shaving of Shagpat.* Moreover, certain important questions receive somewhat casual treatment: the status of totemism, e. g., and the evidence of a matriarchal condition (p. 97). These points must be mentioned; but the criticism does not reflect upon the author, who has taken entirely the right course. "Primitive races, the world over, are changing so rapidly that it seemed well to place on record . . . habits and customs [which], at first in everyday use, showed signs of becoming things of the past. . . . Written in the depths of the Bush, far from every book of reference, . . . this book claims nothing save that it strives to tell the story of a little-known people from a standpoint as near as possible to their own."

The two most striking features of Ekoi life are the organisation of secret societies and the universality of magic. "The whole country is honeycombed with secret societies," of which the Egbo Club is the most powerful. This is a men's club, which has its house in every village, under native rule usurped practically all the functions of government, and possesses a very ancient (partly totemistic) ritual. There are also women's societies, to whose ceremonies men are not admitted; only in exceptional cases, or by a sort of inferior membership, are the societies of the one sex open to the other. Even the children have their mimic Egbo Clubs, to say nothing of 'age-classes' or Junior Republics (p. 283). As for magic, it is "the keynote to which the lives of the Ekoi are attuned." They are animists of the most thorough-going kind; "not only great trees, but the smallest plants possess a soul, and can feel pain when plucked" (p. 287); they reverence ancestors; they have two or three 'deities,' and the author thinks that he has found "traces of an older, purer worship;" but the mainspring of conduct is the juju—which may mean almost anything uncomprehended and mysterious, from a sort of demigod to the 'mana' of herb or stone, including also the manifold means whereby these forces may be influenced or controlled. "Ancestor worship, nature-jujus, secret societies, the principal events of life, and the commonest actions of the day, all blend inextricably in a complicated ritual." Juju dances, emblems, posts, trees, stones,—juju rites, revocations, 'sendings,' 'medicines,'—jujus of good and of evil, of protection and of fear,—accompany the reader throughout the book. The author gives some curious instances of the effectiveness of a juju; for the most part he notes them without comment, but in one case he explains that the "strong-smelling pitch used to 'renew the power' of the juju may offend the nostrils of the keen-scented" leopards against whom it was directed. On pp. 85 ff. is a strange story of the death of a chief owing to the shooting of a buffalo (the chief was a 'buffalo-soul') ten miles away.

The Ekoi show a good deal of artistic feeling in the forms of

their domestic pottery, and are extremely musical. They have, besides a variety of drums (xylophone and regular types), a 'harp,' an instrument corresponding to the Malagasy *valiha* (played by two men, the one of whom "strikes the strings with two slender wooden sticks, while the other touches it here and there with a small closed calabash, with which he makes occasional 'runs' by drawing sharply up and down"), the *okankan* made of two flat bells apposed, rattles, etc. The author gives instances of the drum-language, which Retz—in the adjacent Cameroons—was the first European to master; we regret that he does not allow more space to the subject. He also gives instances of the *nsibidi* or pictorial sign-language, the secret of which was at first jealously guarded (p. 39), but which he has managed, at least in some measure, to interpret.

The book is profusely illustrated by photographs, and by figures in the text. A colored frontispiece shows an Ekoi girl in 'fatting-house' costume. The series of plates representing styles of head-dress (pp. 318 ff.) is especially good. Appendices deal with tabus, clubs, language (grammar, vocabularies, etc.), anthropometrical data, natural history, etc. The author has great sympathy with the people, and has learned to respect them; his work has evidently been a labor of love; and he is to be sincerely congratulated on the amount of first-hand information that he is able to impart.

BOOK NOTES

Stammering and cognate defects of speech. By C. S. BLUEMEL. Volume 1, The psychology of stammering; Volume 2, Contemporaneous systems of treating stammering: their possibilities and limitations. N. Y., G. E. Stechert & Co., 1913.

English writers have long wanted a comprehensive work upon this interesting subject, and we seem to have it in these volumes. The writer discusses mental types, eye- and ear-mindedness, the visual image, brain, relations between mental imagery and voluntary speech, impairment of brain centers, aphasia, stammering, mental confusion, fear and auto-suggestion, respiration, vocalization, articulation, verbal exercises, mechanical appliances, psychological methods, stammering schools and specialists.

Personality. By F. B. JEVONS. N. Y., G. P. Putnam's Sons, 1913. 172 p.

This is an attractive book by an author who has been profoundly touched by the questions which he here discusses,—personality and impersonality, psychology and personality, personality and change, personality and individuality. Bergson and James have evidently most influenced the writer in the preparation of this work.

Educational Psychology. Volume I. The original nature of man. By EDWARD L. THORNDIKE. N. Y., Teachers College, Columbia University, 1913. 327 p.

The title of this work is most challenging. It is the original nature of man which we want to know. The author has treated the subject with freshness and originality but nevertheless with an eye largely turned to practical applications of the doctrines he discusses to education. A fuller notice it is hoped will follow in these pages.

Man and his forerunners. By H. v. BUTTEL-REEPEN. Authorized translation by A. G. Thacker. London, Longmans, Green & Co., 1913. 96 p.

This eminent authority discusses the earliest traces of man; man before the ice age; the glacial and early stone age; the Neandertal race; higher races of the ice age and the Sussex find; men, pre-men and apes; the close of the ice age. The work is illustrated by seventy cuts.

The Canada porcupine: a study of the learning process. By LEROY WALTER SACKETT. Behavior Monographs, Volume 2, Number 2, 1913. Boston, Henry Holt & Co., 1913. 84 p.

This interesting article is based on the study of sixteen porcupines begun at Clark University three years ago. The author has here made a contribution of value to the subject of animal psychology.

Le langage graphique de l'enfant. Par GEORGES ROUMA. Deuxième éd. Bruxelles, Misch & Thron, 126 Rue Royale, 1913. 283 p.

This is the second edition of the author's comprehensive work upon the subject, which along with that of Kerschensteiner occupies the very first rank.

Fortschritte der Psychologie und ihrer Anwendungen. Hrs. v. KARL MARBE. I. Band. Leipzig, B. G. Teubner, 1913. 133 p.

The chief feature of this number is an exhaustive article by Jakob Stoll on the psychology of defects in writing.

On the relation of the methods of just perceptible differences and constant stimuli. By SAMUEL W. FERNBERGER. Psychological Monographs, Vol. XIV, No. 4, Jan., 1913. Princeton, N. J., Psychological Review Co., 1913. 81 p.

Das Seelenleben des Kindes. Ausgewählte Vorlesungen von KARL GROOS. 4th ed. rev. Berlin, Reuther & Reichard, 1913. 334 p.

A clinical manual of mental diseases. By FRANCIS X. DERCUM. Phila., W. B. Saunders Co., 1913. 425 p.

Das Gedächtnis. Die Ergebnisse der experimentellen Psychologie und ihre Anwendung in Unterricht und Erziehung. Von MAX OFFNER. 3d ed. rev. and enl. Berlin, Reuther & Reichard, 1913. 312 p.

Die agrammatischen Sprachstörungen; Studien zur psychologischen Grundlegung der Aphasielehre. Von ARNOLD PICK. I. Teil (Monographien aus dem Gesamtgebiete der Neurologie und Psychiatrie, hrs. von Alzheimer und Lewandowsky, Heft 7). Berlin, Julius Springer, 1913. 291 p.

Denkökonomie und Energieprinzip. Von P. GABIUS. Berlin, Karl Curtius, n. d. 208 p.

Summaries of laws relating to the commitment and care of the insane in the United States. Prepared by JOHN KOREN for The National Committee for Mental Hygiene. N. Y., Nat. Committee for Mental Hygiene, 50 Union Square, 1912. 297 p.

Allgemeine Psychopathologie; ein Leitfaden für Studierende, Ärzte und Psychologen. Von KARL JASPERS. Berlin, Julius Springer, 1913. 338 p.

Twenty-eighth annual report of the Bureau of American Ethnology to the Secretary of the Smithsonian Institution. 1906-1907. Washington, Govt. Print. Office, 1912. 308 + 35 p.

Über "meteoristische Unruhebilder" und "Unruhe" im Allgemeinen. Von MAX LÖWY. (Sonderabdruck aus der Prager Medizinischen Wochenschrift, Vol. XXXVII., Nr. 24, 1912. 106 p.)

The functions of the cerebrum. By SHEPHERD IVORY FRANZ. Reprinted from The Psychological Bulletin, Vol. X, No. 4, pp. 125-138. April, 1913.

The psychology of the personal interview; its relation to moral development through penal institutions. By RUFUS BERNHARD VON KLEINSMID. A paper read before the decennial convention of the Religious Education Association, Cleveland, Ohio, March, 1913. 11 p.

Eugenics and the state. By RUFUS BERNHARD VON KLEINSMID. A paper read before the Cincinnati Academy of Medicine, May, 1913. Reprinted from the Lancet-Clinic. Indiana Reformatory Printing Trade School. 11 p.

The buried animal suture. By HENRY O. MARCY. Reprinted from the Lancet-Clinic, Nov. 16-23, 1912. 32 p.

The selection of stimulus words for experiment in chance word reaction. By ELEANOR A. MCC. GAMBLE and ALBERTA S. GUIBORD. Reprinted from Westborough State Hospital Papers, Series I., 1912. p. 91-109.

Saggi di psicologia sociale. GUALTIERO SARFATTI. Reprinted from "Psiche," Anno II, N. 2, Marzo-Aprile, 1913. Firenze, 1913. 47 p.

Der Ursprung subjektiver Kombinationstöne. Von JOSEPH PETERSON. Reprinted from Annalen der Physik. Vierte Folge. Band 40. 1913. Leipzig, J. A. Barth. 2 p.

L'Année psychologique, fondée par Alfred Binet, publiée par Henri Piéron. Dix-neuvième année. Paris, Masson et Cie, 1913. 515 p.

Proceedings of the Society for Psychical Research, Part LXVII, Vol. XXVI, July, 1913. p. 375-544. Glasgow, University Press.

Proceedings of the American Medico-Psychological Association at the Sixty-Eighth Annual Meeting, held in Atlantic City, N. J., May 28-31, 1912. Pub. by American Medico-Psychological Association, 1912. 511 p.

Internationale Zeitschrift für ärztliche Psychoanalyse. Hrsg. von SIGM. FREUD. I. Jahrgang, 1913, Heft 3. Mai. Leipzig, Hugo Heller & Cie, 1913. p. 205-310.

The moral education of school children. By CHARLES KEEN TAYLOR. Preface by Arthur Holmes. Phila., C. K. & H. B. Taylor, n. d. 77 p.

Dr. Motora and modern psychology. Published by the Memorial Meeting for Dr. Motora. Kodokan, Tokyo, 1913. 470 p. (In Japanese.)

Return of Frank R. Stockton. Stories and letters which cannot fail to convince the reader that Frank R. Stockton still lives and writes through the instrumentality of Miss Etta De Camp. N. Y., Macoy Pub. & Masonic Supply Co., 45 John St., 1913. 314 p.

Miss De Camp was a teacher and in 1909 felt a thrill from her shoulder to her finger-tips and her pencil began to move. After a

number of sittings it began to write, and it finally appeared that she was the medium through whom Frank Stockton, who died years ago, had chosen to communicate further of his stories to the world. They are written and printed under the direction of his ghost and a number of letters of direction from him are printed. Miss De Camp had read Frank Stockton's stories years before, but had almost forgotten them, but here prints seven that were given to her automatically. They are somewhat in Stockton's style, problematical, inconclusive and weird, particularly one which describes the experiences of Mike O'Flynn, who saw his own funeral, rode on the hearse, saw his body buried, crouching over the grave the first night, and finally vanished.

The psychoneuroses and their treatment by psychotherapy. By J. DEJERINE and E. GAUCKLER. Authorized translation by Smith Ely Jelliffe. Phila., J. B. Lippincott Co., 1913. 395 p.

Dr. Jelliffe has done a real service in translating this important work of the great French specialists, who have seen more clearly than anyone else in our day the hygienic value of normalizing the emotional nature. Their therapy does not consist chiefly, like that of Dubois, in straightening out the intellectual processes but in adjusting the sentiments and emotions. To our thinking the very best part of the work consists in the first chapters, which show mental effects of disorders of the digestive, urinary, genital, respiratory, circulatory, cutaneous and neuro-muscular systems. In very many of these disorders the writers find a genetic factor. The book is of great value not merely to physicians but to psychologists.

A scout of to-day. By ISABEL HORNIBROOK. Boston, Houghton, Mifflin Co., 1913. 290 p.

This attractive little book is a story for boys by the author of "Camp and Trail" and other stories, which shows the purpose and value of the Boy Scout Movement. One of its interesting features is the song for the Scouts, which is being adopted for use by some troops. As the book is a story it can not be reviewed here at any length but it should prove interesting to all Scouts and all adults interested in the Scouts.

PSYCHOLOGY AND PHILOSOPHY

The following announcement is taken from *The Journal of Philosophy Psychology and Scientific Methods* for June 5, 1913:

The subject of "The Standpoint and Method of Psychology" has been selected as the topic for the joint discussion of the American Psychological Association and the American Philosophical Association at its next meeting at New Haven. Professors Edward G. Spaulding and Howard C. Warren, of Princeton University, suggest the following formulation of the problem, which, it is hoped, will serve as a starting point for further formulations and discussions:

Data of Psychology.—Should psychology study unit-beings (selves, mind, consciousness), or inner states (*e. g.*, sensations feelings), or inner processes (*e. g.*, sensibility, affectivity, association), or certain relations between unit-beings and their environment (*e. g.*, reflexes, instincts), or several of these?

Method of Research.—Should the psychologist obtain his data mainly by self-study (introspection by himself and others), or by studying the motor reactions of organisms? If both methods be admitted, what is their relative importance?

Philosophy of Psychology.—Does a systematic psychology depend upon a specific world-view, or can it be developed, as are physics and biology, without a definite philosophical basis? In the latter case, do the results of empirical psychology compel us to adopt some specific philosophy?

Note.—The question of the nature of consciousness, sensation, introspection, etc., should be discussed only in its relation to the standpoint that is taken concerning the above positions.

A frank discussion of fundamental issues, by men who are pursuing different lines of research and have been trained in different disciplines, will often clear the air of needless misunderstanding, and may contribute more positively to the advancement of science. Nor can anyone be more welcome in such a debate than the philosopher, the 'spectator of all time and of all existence'; for he brings an historical perspective which the man of science too often lacks, and he weighs hypotheses with an impartiality which the special student too rarely attains.

It is, then, because I am in harmony with the spirit of the Note printed above that I am moved to enter a protest against the form which has been given to the subjects proposed for discussion. No one, not even the philosopher, may legislate for a growing science, and say what its data and methods 'should' be. Psychology makes its way through the tangle of experience by what methods it may, and gathers as data what facts it can; a new method may enlarge its scope, a novel observation may open up a whole field of work. Only when a science is perfect, and the life has gone out of it, can its data be circumscribed and its methods defined. Psychology as a science is still in its childhood, while its task is as immense as that of physics, the mother of the sciences; it appeals to all the temperaments, and satisfies the most diverse interests of man; to direct it by 'shoulds' and 'should-nots' would be, in my judgment, to hamper its growth and to check useful effort.

E. B. TITCHENER.

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